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"For many fortunate discoveries in medicine, and for the detection of numerous errors, the world is indebted to the rapid circulation of Monthly Journals; and there never existed any work to which the faculty in Europe and America were under deeper obligations than to the Medical and Physical Journal of London, now forming a long, but an invaluable series."—Rush.

To the Editors of the Medical and Physical Journal.

(With a Plate.)

GENTLEMEN,

THE history of the annexed case is as follows. The subject of it was a middle-aged widow, and mother of several children.

Above twenty years ago, a tumor arose the size of a small pea, without any apparent cause, below the ear. The increase was slow and gradual, confining its dimensions for ten years to one small point. After that period other swellings commenced, until the size of the whole became such as is represented by the drawing. The weight and consequent pressure of the tumor produced some occasional vertigo, which was the sole inconvenience to the patient.

This case became unusually interesting, inasmuch as it was the opinion of a great number of respectable physicians and surgeons who had seen it, that an operation, should it ever be deemed practicable, would be attended with imminent danger; and the most unfavorable prognostic had been given by several others of the inevitable result of an operation.

For my own part, whatever apprehensions I might have as an operator, from the situation, condition, and circumstances of the case, yet, desperate as it was, my own opinion inclined decidedly for the operation. It was considered, in consultation, as practicable, however hazardous, and it was determined that if the patient chose to undergo the operation, and I had no objection to undertake it, that opinion should be my sanction.

The poor woman, in contemplating the situation and probable result of her case, as represented to her by the consulting gentlemen, evinced a firm and sensible mind.

Having a numerous family to rear under narrow circumstances, she had met and borne the inconvenience and difficulty of her malady with great fortitude. After two and twenty years' industrious exertions, she had managed to

bring up her children. They were placed and settled in safety: of course she found herself at liberty to consult her own feelings, and, if necessary, to abide by any risque in relieving them. Whatever expedient she might choose to try, now solely implicated herself. With this view she sought advice, and the operation being adjudged indispensable, she submitted to it patiently.

This was at any rate an awful decision, and it may be presumed that to confront the terror of an approaching dangerous expedient, in the face of numerous unfavorable forebodings, and of as numerous friends and visitors, would require something superadded to ordinary courage; for the size and appearance of the tumor rendered the patient an object both of opinion and of curiosity.

The operation was, of course, painful in proportion to the large extent of incised surface. Care was taken in performing it to preserve as much skin from the base of the tumor as should afford a covering to the wound. The incision was begun in that part of it by which the effusion of blood would least incommode the track of the scalpel. The inequalities consequent upon the lobular structure of the mass, had impressed some deep indentations within the neck; and the weight and bulk rendered it both unwieldy and inconvenient in operating.

On extirpating the tumor, a considerable arterial branch was unavoidably divided, which on the first jet of blood, and in that situation, had a formidable appearance. It was promptly restrained by pressure of the finger, until the operation was so far advanced as to make it convenient to secure it. A ligature was applied before the base of the tumor was separated, to prevent the occurrence of hemorrhage.

The tumor being completely removed, the carotid artery was observable in its course up the neck, exposed and pulsating. The large corresponding jugular vein presented also its broad, thinner, and less conspicuous coat. This became an object of notice only when certain compressions from motions of the neck allowed it to appear full, or to be emptied.

The incisions were obliged to be carried in two or three angular directions on the neck and cheek, yet they formed eventually a compact cicatrix; and the parts being carefully attended to during the healing process, left no very particular deformity. They were united and well in five or six weeks after the operation. The mouth, however, was rather disfigured from a certain unavoidable division of the nerves, and of course from a corresponding abatement of their influence upon the muscles.

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The weight of the tumor was three pounds and nine ounces.

The patient had not any material bad symptoms during the cure.

The substance of the tumor consisted of a condensed smooth firm structure on the outermost lobes; and of a more pulpy, darker, greasy mass in the middle ones.

I have the honor to be, &c.

JAMES ATKINSON,

Surgeon to His Royal Highness the Duke of York, Senior Surgeon to the York County Hospital and Dispensary.

To the Editors of the Medical and Physical Journal.

GENTLEMEN,

IN looking over your Journal for the present month, I find that Dr. Yeats reserves to himself the liberty of choosing the time and season for publishing the case of Ann Foulkes.

That the proper time for publishing her case was the period when his paper upon Ischuria made its appearance, will be apparent to every one, I humbly conceive, who peruses the following statement—that his conduct towards me and others in not doing so has been very extraordinary, and that we must necessarily feel hurt at his endeavours to reflect by implication upon our professional conduct, must be equally apparent.

Early in the spring of 1812, I repeatedly heard that there was a most wonderful case of disease in this town, and that the subject of it, A. F., evinced the most pious resignation under sufferings almost unparalleled.

I knew not whose patient she was, or had been. Motives of professional curiosity induced me to call upon her. I first did so on Thursday the 19th of March, and found her suffering apparent uneasiness from a tumefaction of the epigastric region. She informed me it was occasioned by a collection of urine, which was occasionally evacuated by vomiting. That every second or third day the accumulation was so great as to oblige her to take an emetic, which always enabled her to discharge the urine by the mouth very freely; but that if she did not take the emetic, her sufferings were much increased.

This state of things, she said, had existed about twenty weeks; and that during the whole of that time she had not voided a drop of urine by the natural passage.

Her general health did not appear to have suffered so much as might have been expected, from such an extraordinary, and, I may add, almost incredible disease. Indeed it struck me as being wonderful that she evinced so few

 symptoms

symptoms of general illness, considering she had been so long confined to her bed.

This circumstance I wish to direct your attention to in a forcible manner. We all know how easy it is to feign numberless pains; but we as well know that long-continued visceral disease does universally show its effects in a direct and obvious manner.

Added to this, I have no hesitation in declaring that her digestion was at this time very healthy and perfect, from the well nourished state of the body, and from an inspection of well formed and healthy-colored fæces. It appeared to me very strange that these things could be so, whilst she complained of the dreadfully irritating effects of the discharged fluid upon the throat, fauces, and mouth. To satisfy myself upon this subject, I now thought it right to make every possible inquiry into the previous history of her diseases, and to pay great attention to these extraordinary symptoms. The more I did so, the more had I reason to fear there was some incorrectness in the relation of her story.

On the following day I called with a medical friend upon whose judgment I placed great reliance, and his astonishment was also excited.

We agreed to attend alternately at the periods of her taking the emetics. Previously, however, to doing this, we thought it essential to examine the state of the bladder by means of a catheter, and were *very much surprised* to find that no examination of this kind had been made in the early stage of the disease. To our great astonishment, both A. F. and her mother most obstinately refused to permit this simple operation. Our suspicions were then strongly excited, the more so, because they were very loud in their assertions of there being not a drop of urine in her bladder. They likewise affirmed that there had not been a drop of urine in that organ for several months. To strengthen this declaration, they assured us that Dr. Yeats, who had attended her very assiduously in the early period of her present illness, was of the same opinion. She was now attended solely by a druggist of this town, who, with the sanction and full approbation of Dr. Yeats, prescribed emetics, diuretics, &c. What steps were taken to assist in the ejection of the urine before Mr. Palgrave, the druggist, ordered emetics, I know not. Although it was represented that her sufferings were very dreadful at the time the urine first stopped, it was very extraordinary that her symptoms were not such as usually attend retention and suppression of the urine.

Upon a further and more accurate examination of the

parietes

parietes of the abdomen, I now discovered that the muscles of its lower part were always put into violent action, when the tumefaction of its upper part was most apparent, and in vain I requested that they might be relaxed. This, of course, led me to suspect, that the said tumefaction or inflation was in some measure dependent upon the action of these muscles, and I was certain that their action was principally voluntary. The truth of this suspicion was made very apparent some time afterwards; I happened to call at a period when she did not, or could not contract the muscles so forcibly, and the tumefaction had disappeared. Upon an examination of the epigastric region under these circumstances, I could discover no trace of visceral disease.

I had been assured the swelling always increased in proportion to the supposed accumulation of urine; but at the time I now allude to, she had not vomited urine, according to her own report, for some days, and I could only account for the absence of the swelling, by the quiet state of the muscles below. The hypogastric region was at this time fuller than usual, and I suppose she was incapable of practising her common habit of blowing out the epigastric region, owing to there being an unusually full state of the urinary bladder.

I had the less reason afterwards to be surprised at Dr. Yeats' believing that the tumefaction was now owing to a real collection of urine, because he did, in the most positive and unequivocal manner, declare his utter disbelief of the possibility of any person being able to cause such an inflation as I constantly witnessed, whereas nothing can be more easy.

There was no doubt of the nature of the fluid which the girl and mother positively asserted she (A. F.) vomited up. It was clearly urine. It now remained to be proved that such fluid was actually discharged by vomiting.

My friend went to her house on Saturday morning the 3d of April; her mother said she had just swallowed the emetic, and that it was beginning to operate. He staid a considerable time, during which she appeared to be very sick and retched often, but neither urine, nor any thing of an urinous smell was brought up. With much persuasion he induced her to swallow some warm water; this returned by vomiting, but quite free from any admixture of urine. He then left her, perfectly convinced of the incorrectness of the whole story, as he was before assured of its utter improbability.

A few days after I went to the house, whilst she was said to be under the operation of an emetic. When I entered the chamber, her mother shewed me about half a pint of fresh, transparent, and apparently healthy urine, free from the

the least admixture of mucus or any other fluid or solid matter, which she informed me her daughter had just vomited up. On looking at it, I felt quite sure it could never have been discharged from the stomach or throat of a person under the influence of an emetic, because of the total absence of that matter, which must necessarily be in the stomach, throat, or mouth, of every living person. This quantity was, however, very small, compared to that which I had been informed always came up when an emetic was taken; I therefore waited patiently for more urine to make its appearance—she professed to be very sick, but none came.

Her mother then told me it was not a vomiting day, and that some days she did not vomit up the urine so freely as she did on others; contrary to the story they first told me, when it was not supposed I should be sufficiently inquisitive, to insist upon seeing this urinous matter vomited.

At this time the mother likewise expressed a hope that I was satisfied what she had showed me was urine. I certainly was, but equally so, that it had been discharged from the body in the natural manner.

To put the matter to rest, I now spoke in a very decided manner, about the propriety of allowing a catheter to be used. I pledged myself that she would be cured by it, but no persuasion was of any avail. I afterwards called at the house, and expressed such displeasure at their continuing to refuse the assistance which I offered to procure, as made them endeavor to excuse themselves under the cloak of religion.

Upon offering to send for Dr. Yeats, or any other medical gentleman they might prefer, in order to make the examination with the catheter, they now told me it would be of no use, because they were both determined not to submit to it—being quite sure it was wrong to try to take the work out of the hands of God, who had thought fit to afflict her in this very extraordinary manner: and both A. F. and her mother declared they thought it the duty of the former to wait God's time for bringing about a cure, if it was ever to take place. They appealed to me upon the harshness of my conduct, in appearing to disbelieve a person, who was so generally respected for the piety of her character; and said, that I ought to be ashamed of myself, (or words to that effect,) for withholding my assent to those reports which Dr. Yeats, and many other persons, implicitly believed, upon their simple assertion. This determined me to inform her charitable supporters, and the overseers of her parish, of my doubts respecting the veracity of her story;—and I hinted to them what was my determination, in case

their free consent was not soon given, to the proposal above stated. I was, however, prevented from doing so, by receiving information from the mother of A. F. of a great change in her daughter's complaints. She was said to be seized with symptoms of common fever, which was accompanied with apparent stupor, of two or three days duration, and at the close of this symptom, the urine found its way from the stomach to its proper organ, from which it has been regularly discharged to this time. I once saw her during this declared stupor, which I considered as deceptious, from the absence of those symptoms which are its usual concomitants; neither was any change or derangement of the urinary organs apparent.

Before the asserted return of the urine to its natural channel, I felt fully assured that a proper examination of the bladder would prove the total falsity of A. F.'s tale, and upon that account I thought it my duty to insist upon its being made. Probably the parties concerned well knew the same. Nay, I add, it is very unlikely that a case so painful as retention or suppression of urine has ever existed, in which the sufferer objected to the proper methods of attempting to give relief.

I must now touch upon some circumstances connected with this second Tutbury tale.

It has been objected to my opinion of the case being a clear attempt at imposture, that there was no adequate motive. I am not bound to prove any motive, but I can venture to assert, that it would astonish most persons at a distance from Bedford, to learn what was the greatest quantity of provision, clothing, and money, sent to A. F.'s house in one month, previously to my declaring this opinion publicly.

But for the circumstance of Dr. Yeats being much implicated in the foregoing statement, I should have sent you the result of my inquiries into this reputed state of Ischuria long ago. With the greatest reluctance to become personal, I feel obliged, in justification of my own character, and the opinions of other medical men, to make some observations on the papers which have appeared in your Journals since April last. As Dr. Yeats all along did, and I suppose to this moment does believe, that this marvellous vomiting actually took place, he, of course, felt an obligation to controvert the opinions which I and other medical men gave upon the subject, namely, that the declarations of Ann Foulkes and her mother were untrue, and utterly impossible.

I, and many others, distinctly understood from Dr. Yeats, that he intended (or pledged himself) to prove both the truth and

and possibility of A. F.'s story. The abstract question of vomiting urine for a length of time was never thought of, unconnected with the peculiarities of the case in question. Is it not then very extraordinary, that Dr. Yeats should try to reflect upon our opinions, by implication, in a manner which is unfair? But for the occurrence of this case, his paper upon Ischuria would never have been written. When I first saw A. F., and during the whole time I visited her, previously to the asserted return of the urine to the bladder, she was attended by Mr. Palgrave, a druggist, of this town. It is strange to tell, that Dr. Yeats gave up the care of this interesting case to him alone. Mr. P., under the controul of Dr. Yeats, gave the emetics and gave the diuretics. Without calling in any of the medical men of Bedford to assist him in investigating this wonderful story, Dr. Y. rested satisfied that all he heard was true.

When my mind was decidedly made up on this subject, and whilst I considered A. F. as being more immediately under the care of the druggist, I told Dr. Yeats, in a truly friendly manner, that I was in possession of such facts as compelled me to consider her in the light of an impostor. He then seemed to think that my opinions were not founded upon facts. He assured me, that he paid so much deference to the assertions of the parties interested in this (as I supposed deceptious) affair, that he could not think of changing his opinions respecting her case. In addition to the facts which he thought would satisfactorily prove the truth of all this marvellous story, he is now in possession of the oaths of two persons who were in the habit of waiting upon or nursing A. F., which oaths I may hereafter show are to be regarded in a very suspicious point of view.

Not only did I inform Dr. Y. of my opinion, but I sent him a written statement, containing the substance of the foregoing history. He, in return, was kind enough to favor me with the perusal of a very long account of her case, which I understood was intended for publication; and, if I am not misinformed, has been already presented to a medical society for publication. If I had wanted any information to strengthen my previous opinion, or clear up any remaining doubts, the inspection of Dr. Y.'s case-book would have been amply sufficient; it corroborated, in a remarkable manner, what A. F. had previously told me about the absence of symptoms, which a better informed impostor would have spoken of when mentioning what took place at the time she said the urine first stopped.

The case now appeared so clear to me, and all those medical men with whom I had any intercourse or correspondence,

ence, that I could not think it probable Dr. Y. would venture to commit himself by writing upon the subject, except it was to acknowledge that he might have been imposed upon: still less, after what had passed, could I suppose it possible he would write upon Ischuria, or vomiting of urine, without giving the particulars of this case.

It must be evident to every one, that if I say A. F. is an impostor, because of the utter impossibility of her having vomited urine in the manner reported above, I have a right to expect that the man who controverts my opinion will do it in a fair and candid manner.

It was unpleasant enough for me, that in the simple discharge of my duty, I was obliged to incur the ill opinion of many well-meaning persons, whose good opinion I have been always solicitous to deserve; but that Dr. Y. could go such a round-about way to work to try to make a fact of what I suppose a fable, astonishes me much indeed. I have to assure you, that it was principally out of tenderness for his professional character, that I abstained from publishing my opinions of this case before; and I now feel justified in calling upon him, through the medium of your Journal, to give his history of the whole case, or acknowledge that he was imposed upon.

Dr. Y. might have chosen the time and season for publishing upon Ischuria; but I must repeat, that if I did not call upon him to publish this case, according to his promise, I cannot willingly submit to his assertions in your Journal for the present month. The point in dispute between him and me, and I believe between him and other medical gentlemen, is, simply, whether it was possible for Ann Foulkes to have vomited urine in the manner, and for the length of time, she declares she did. I do not act more unfairly towards Dr. Y. in relating the following story, than he has done towards others and myself, in writing his paper upon Ischuria, without giving the case of A. F. in connection with it.

A woman, who lived in Essex a few years ago, declared, as did her friends, that she was in the habit of sweating a vast quantity of living tadpoles, or animalculæ, of a visible size, which were seen very merry in the buckets full of fluid that was said to have exuded from her skin. This excited the wonder and curiosity of the neighbourhood. When it was understood that a medical gentleman believed it, many persons went to see this phenomenon from a distance, and made liberal contributions towards defraying the expence of washing and drying the great number of cloths that were used to absorb this unusual discharge from the skin, and to

furnish her with wine to keep up her strength. At last, a physician of great respectability insisted upon seeing these animals make their way through the skin. That could not be acceded to. I scarce need add, that, upon further inquiry, it was proved, the persons in connection with this woman supplied her with tadpoles from a neighbouring ditch. I am, Gentlemen,

Your obedient Servant,

Bedford, Sept. 9, 1813. J. GIBBON.

To the Editor of the Medical and Physical Journal.

SIR,

IT may appear somewhat to border on presumption, to undertake to maintain opinions contrary to the tenour of one of the most respectable publications on a medical subject that I am acquainted with, whether we view it in regard to style, or the candor with which it is published, or the well-deserved reputation of the author, supported also as it is by some present authorities of deserved influence in the medical world: I refer to Dr. Haygarth's second edition on the subject of Acute Rheumatism. Such a weight of authority that has now engaged in this cause, and adopted the practice which is the particular subject of the volume, must tend much to fix those who may have been wavering in their opinions respecting the cure of that disease, and also those who can have little else to guide them, beyond a fair application of the practice which they are taught through lectures, or which they learn from books. For myself, I should have been very happy if I could have received information through so respectable a channel as the present author, which had perfectly convinced me. But as that is not the case, I feel more inclined to hazard my feeble opinion in a somewhat contrary direction to this respectable publication.

We have very plainly seen, in the instance of Sydenham, in what strong chains he has held the medical world, in respect to his opinions on the Gout, which was one of the last of his performances, when his reputation had arrived at the highest pitch of credit. And in the same way we may be likely again to be bound down for many years to a practice coming in the shape of most overwhelming authority.

The reputation of Dr. Haygarth has deservedly arrived at the highest degree of eminence; and if with such reputation he promulgates opinions, they are likely to make the deepest impression. My experience has, however, led me to conclusions different from the publication alluded to; and

and these conclusions, as they appear to me important, under the present state of the question, I shall most freely impart.

I have for many years, at various times, adopted the practice which Dr. Haygarth recommends; but I am sorry to say I could never conclude very favorably of it. It must not, however, here be understood, that I have seen much evil ensue from it; but it has never clearly appeared to me to be followed by any decidedly salutary effects. I am persuaded it may happen, that cases of acute rheumatism will be very short under various modes of treatment; but this does not prove the exact efficacy of any of these plans as a remedy for the disease generally.

Dr. Haygarth has given a case of a quick recovery from this disease, by the use of the Peruvian bark; and this very patient had been at some previous time ill of the same disease, under a different treatment, for many months. I will give a parallel case, under a different method of cure.

A robust seafaring man had been taken with a shivering, followed by an attack of fever, with pains in his limbs, three days before I saw him When I visited him, he was in the most helpless state imaginable, with rheumatism, and swellings in every limb and almost every joint. Two years before this attack, he had suffered from the disease through fifteen months, which incapacitated him for any employ during that time. He had not been bled in the first attack; but I now directed twenty ounces of blood to be immediately drawn; he was also smartly purged with neutral salts. I saw him two days after this, he was much better. I however again directed blood to be drawn, and that he should again be purged. I visited him two days after this, when I found him up, with very little rheumatic pain; and in the course of a week he came to my house, a walk of some distance, and informed me he intended immediately to go to work.

This was as severe a case of acute rheumatism as I ever saw, and was as rapidly cured. If, indeed, we could always succeed so well in the cure of this disease, we might be said to have found out a certain remedy for it; but since this, I have never seen so quick a recovery from the same means, and therefore I do not think the plan pursued in this case entitled to be considered as a guide for the cure of this disorder generally. I think, also, I have seen very beneficial effects to follow perspirations; but they by no means frequently carry off the disease, and therefore I should judge sudorifics not to be always actual remedies for acute rheumatism.

I am very glad Dr. Haygarth has given this edition of his book

book to the public, because I think he has been more explicit in one important point than he was in his former edition, at least so I am impressed, for I have not his former work by me. The tenor of that, in so far as my mind is impressed, was rather to dissuade from blood-letting, and to trust to the use of the bark. At present I am pleased to find that the bark is to be left off, in some cases, to give way to blood-letting, and after a time to begin it again. In regard to blood-letting, in so far as my opinion goes, it appears to me to be almost indispensable: I do not say that it is a positive remedy for the acute rheumatism, but I think it ought almost always to be used to prevent some of the serious consequences of the disease, if we were not quite certain that it would contribute towards its cure.

Early in my medical career, I happened to be placed in a neighbourhood where the prevailing practice in this disorder was that of copious bleeding and purging, with antimonial and saline medicines. I do not say that by this mode of treatment the diseases were not often very obstinate, but they certainly maintained the character of being less inimical to life than I have found them of late years; and, indeed, according to Dr. Haygarth's statement, acute rheumatism is full as fatal as fever, if in addition to this we take into the account those who are attacked afterwards with diseases of the heart, which are very numerous. I had the misfortune to be called to three rheumatic patients within a very short space of time, who died of the rheumatic phrenitis, and all of whom might with propriety have been bled. I am strongly impressed that these events would not all have happened, had that operation been gone through. I have also seen numerous cases of rheumatism of the heart, and only in one instance could I discover that the parties had been bled, and that one in no considerable degree. My experience, therefore, leads me to infer strongly, that these fatal events and serious diseases would have been mostly avoided by the liberal use of the lancet, or other means of losing blood. I am, therefore, as I say, glad to find Dr. Haygarth keep in view and recommend the remedy of blood-letting, more than his other publication appeared to me to do.

One evil arising from the use of the bark, in London and in its vicinity, is, I fear, the almost total neglect of the lancet in this disorder, which I trust will be more revived. Theoretically, in fact, there seems to be a contradiction between the remedies of the bark and blood-letting, and I am persuaded this seeming opposition has had great eff t in preventing the use of the lancet.

Having

Having said as much as may be necessary at present on the subject of blood-letting, in so far as Dr. Haygarth and myself are concerned, I must now advert to antimonials and sudorifics. I consider, in an early stage of the complaint, that it is desirable to procure perspiration, nor is that difficult. With this intent antimonials, joined to saline medicines, may be very proper; for, as I have said before, I believe I have seen cases in which perspiration occasioned very material relief, though these are not numerous: but I have never seen perspirations continued for a long time attended with any advantage, if obvious benefit has not ensued within the first twelve hours; but when they have been protracted, they have appeared to be decidedly detrimental, and therefore not to be encouraged. The use of antimonials, therefore, beyond this period, seems to me to be of no other advantage, than in so far as they render the bowels soluble; but in so far as they encourage perspiration, by no means desirable: in fact, it has been my practice for some time absolutely to discourage them, by recommending any flannel wrappings to be laid aside, by prohibiting any warm drinks, and by desisting from the further use of antimonials. To curb this state of perspiration, I confess I have not seen the bark sufficiently effectual, and I am certain I have often seen it increase the inflammatory symptoms, and materially aggravate the pain. I might perhaps allow, that in such circumstances small doses of bark may be less detrimental than sudorifics; but that is all the praise I can give that medicine.

In regard to the use of anodynes, I can say nothing, from my own experience, that warrants me to conclude favorably of them generally, though I have occasionally seen some good effects; but from what I have observed, if 20 drops of laudanum is not attended with evident advantage, no better success will generally ensue from a larger dose, but frequently an exacerbation of the night paroxysm to a considerable degree. I have for many years agreed with Dr. Haygarth, and Dr. Currie of Liverpool, that acute rheumatism is one of the most tedious and intractable of diseases: I feel inclined, however, of late, much to modify that opinion, and to a certain degree to reject it.

The means of cure of acute rheumatism which I have hitherto mentioned, have appeared insufficient to satisfy me that something further ought not to be attempted. In respect to the management of the patient, in discouraging perspirations I considered that I had gained something, and therefore I not only recommended every local means to be avoided that were likely to increase that secretion, after a time

time alluded to, but also to abstain from such medicines as might promote it. Antimonials and saline medicines have been, therefore, recommended to be laid aside; and the vitriolic acid, in the form of Infusum Rosæ, to be given. Epsom salts have occasionally been added to this; or if the bowels happened to be open enough, or other aperient medicines employed, six or eight grains of nitre were directed to be given with each dose. In this way I was better satisfied to conduct the treatment of the disease than by the use of antimonials or the bark. Lastly, I advanced another step, which was to adopt the use of local cold, which has been effected by camphor mixture, and which I was principally induced to use on account of the advantages derived from local cold in peritonitis, as stated in my tract on that disease.

This then, in addition to the other means which my observation has taught me to approve, appears to conduct the disease to a speedy and safe conclusion; and they altogether seem to me to be well entitled to be considered as positive remedies for acute rheumatism.

Under the supposition, therefore, that I was called immediately on an attack of this disease, I should recommend blood to be drawn to some extent, to open the bowels, and to give antimonial and saline medicines. On the following day, if the symptoms of general fever were not abated, I should again direct the use of the lancet. If the antimonial and saline medicines had produced copious perspiration without material relief, I should, on the third day, order them to be desisted from. I should now recommend the vitriolic acid, with constant attention to the bowels, and the use of camphor mixture to those joints which were the most painful; and all along prohibit the use of flannel and hot applications to the pained parts, or the warm bath; and to keep the body only moderately covered. I have been better satisfied with the use of a dose of four or five grains of Pil. ex Aloe cū myrrh every six hours, and an occasional purgative of a more active quality, such as castor oil or salts, by which the bowels are kept in a sort of constant activity, after the first days of the attack, than by the use of occasional active purgatives. Through this mode of proceeding I find very little difficulty in subduing acute rheumatism in its ordinary form, and I am able to state confidently a material change for the better in a week or ten days, if not a total subjugation of the disease.

I shall now mention some of the last cases of this disorder that have been conducted upon this plan.

Case 1st.—A person of about 35 years of age had been eight days ill of acute rheumatism. He had been bled in the

the commencement of the attack, the bowels opened, and saline antimonial medicines given. He had also taken some opium without relief, and was much wrapped round with flannel. . During this time his pains had not abated, but the perspirations had been profuse, and he had been in a state which his friends thought approached to delirium on the night preceding my visit, and was not quite collected when I saw him. As I have a great dread of rheumatic delirium, I did not feel easy for the event of this case. I, however, directed all the flannel wrappings to be laid aside, sixteen ounces of blood to be taken from the arm, and a medicine composed of Infus. Rosæ, with a sufficient quantity of Magnesiæ Sulphas. On the next day the patient was somewhat better; had passed upon the whole a better night. I, however, judged he ought to lose more blood to the extent of eight or ten ounces, the other medicine to be persevered in; and, as the patient mentioned something of a cold application, I directed camphor mixture to be employed to the parts most in pain. On the following day my patient was much better, had passed a good night, and was in very fair progress in amendment. In four days after this, being a week from my first visit, I found him dressed, and rapidly recovering, which was afterwards uninterrupted.

Case 2d.—A woman who had suffered from an attack of that species of puerperal fever which I gave an account of in the Edinburgh Medical and Surgical Journal, and who had recovered under Mr. Butler's care, at Woolwich, in the manner I had described, was, about two months after that attack, seized with acute rheumatism. When I saw her she was in the most excruciating pain. She was much wrapped round with flannels, and in the most profuse sweat. The disease occupied the whole of her limbs completely. As she had suffered from diarrhœa under the puerperal attack to a considerable degree, it was not thought right to bleed her. The whole of the flannel wrapping was directed to be laid aside, and the camphor mixture to be applied to some of the pained parts first, and the bowels to be kept open by saline purgatives. I found from Mr. Butler, three days afterwards, that all my directions had been complied with, and that the patient was so much better as to be up and dressed, and in the course of a week she was so well as to entirely leave off medicine.

Case 3d.—A young gentleman, æt. 13, after being much heated by violent exercise, was taken, on the following day, with a cold shivering, succeeded by fever, with pains in his joints. On my second visit, I found the disease to be acute rheumatism, by the increase of pain, swelling in his knees

and

and ankles, and fever. As the swelling came on in the evening after I saw him, his friends wrapped them round with flannel, and by the aid of the saline mixture and James's powder he had been in a profuse sweat for sixteen hours, without any abatement of the symptoms. On my visit I directed the flannel to be discontinued, and the parts affected to be kept moistened with camphor mixture. Two large leeches were also applied on the back of each hand, which bled very considerably for many hours; and Epsom salts in Infus. Rosæ were directed to be taken. In two days the fever and pain were so abated that I found the patient up and dressed. A pill with three grains of Pil. ex Aloe cū Myrrh, to be taken twice a-day, was now directed; and the Infus. Rosæ with nitre. The patient had no further return of the disease, but continued to wrap the joints affected round with cloths, moistened with camphor mixture, for some time, at nights going to bed; and he was so sensible of the relief he experienced from this application, in comparison to his sensations under the flannel, that he was afterwards very desirous of using it. About a fortnight from this time I was desired again to visit the patient, who complained of some weakness in his ancles, and languor. Upon examining into his state of health, I found rather too much quickness of pulse, with a sharp stroke, and some palpitation at the heart. In the intervals of my visits, he had taken the bark in doses of ten grains three times a-day. I directed five grains of Pilula Ferri cū myrrh to be given twice a-day with some infusion of gentian, and the bowels to be kept open with castor oil, or equal parts of rhubarb and jalap; and the patient soon became better, and went to school; but, upon his again complaining of languor, he was sent to the sea side to bathe, where he most completely recovered.

Case 4th.—A gentleman, about 40 years of age, of a delicate frame, after visiting some damp apartments, was attacked with pain in the right shoulder, accompanied by a severe shivering and fever. Some years ago he had labored under acute rheumatism, and in one of the attacks had been confined six weeks. Latterly he had suffered some pains in his joints, which he had been able to remove by the help of a flesh-brush, which he applied also on this occasion, but without relief. He then had recourse to an old prescription, with volatile tincture of guaiacum and thirty drops of laudanum, to be taken at night, which had occasioned no relief. On the fifth day of this attack he sent for me. He had passed a very bad night; the shoulder was in great pain; he had a coated tongue, with much febrile heat, and a pulse of 108. He had also perspired very freely without relief, and had involved

involved the part affected in flannel, which only caused an aggravation of his pain. He had taken some castor oil on the day I saw him, that had acted on his bowels, notwithstanding all which the disease was gaining ground. I directed the flannel to be removed from the part, and six leeches to be applied, and encouraged to bleed freely, which, on the following morning, I found took place. The Pilula ex Aloe cū myrrh, in four-grain pills, was directed to be taken every six hours, with a medicine composed of infusion of senna and barley water. On the next day the patient was somewhat better, but had still considerable pain. I now directed the camphor mixture, which was sparingly used, but the patient was relieved. His fever was gone, and only a considerable degree of stiffness in the part, and numbness of the whole limb. I still persisted in advising a more free use of the lotion, and on my visit at the interval of a day, (the fifth from my first seeing him,) I found he had in some measure complied, and was then capable of using a pen, and elevating the limb from the side at a considerable angle. The patient's expression now was, that every time he used the lotion it loosened the joint; and he found himself so well as to require no further attendance from me.

These then are the histories of the last cases of acute rheumatism that have occurred to me; and the quick recovery of the patients justify me at least in the conclusion, that the disease could by no known means have been more speedily removed; and, so far as my observation can guide me, I conclude that neither the bark, nor antimonials, nor any other plan of cure that has hitherto been recommended, could have succeeded so well. This, therefore, stamps its value. Future observation, perhaps, may lead to a still more speedy and efficacious method of treatment, which I shall be happy to witness.

This hasty account, which, however, I hope is such as to be completely intelligible, I have been thus induced to give at the present moment, when the medical world are likely to receive information on the subject of rheumatism with increased interest, on account of Dr. Haygarth's publication. As I cannot accede to the practice he recommends, I think it my duty to communicate my dissent with every respect. I trust I have stated nothing that will fail in other hands; and if it should happen so, I am sure Dr. Haygarth will be the first to excuse me in this attempt to invalidate his practice in rheumatism. If this should completely succeed, he has such reputation in store as ought to satisfy any one, and which is built upon an immoveable foundation, and has stood the scrutiny of time.

 It

It might have been expected that I should before have taken notice of the author who has much preceded me in the use and recommendation of cold applications to the parts affected in the disease now under discussion; but I considered the most natural mode of statement to be that which connected the progressive stages by which I arrived at the treatment I now confidently recommend. Dr. Kinglake's book and opinions are before the public, which certainly settles his priority of claim in this respect. Without pretending to any discovery, my whole object is to put the public in possession of the most material observations I have made, through a series of years, towards the best mode of subduing acute rheumatism.

Greenwich, THOMAS SUTTON, M.D.
Sept. 25, 1813.

For the Medical and Physical Journal.

On the NATURE *and* TREATMENT *of* APOPLEXY.

(Continued from p. 197.)

Memoir the Second, by PORTAL.

THE Observations on the Nature and Treatment of Apoplexy, which I communicated to the Academy of Sciences, and which were printed in their volume for 1781, have proved, that many apoplexies which had been thought serous, were nevertheless sanguineous; and hence conviction must arise, that the signs by which physicians thought to distinguish the sanguineous from the serous apoplexy, such as paleness of the countenance, weakness, smallness and slowness of the pulse, were illusory, and had led physicians to the most baneful practice in the treatment of this malady, since in place of prescribing bleedings, which were necessary, they gave vomits, which could easily prove fatal.

Facts so important, and furnished by anatomy, could not but be useful to medicine. I have become more bold in prescribing bleedings in cases of clearly-marked apoplexy, not in the sanguineous only, in the treatment of which physicians of all ages have agreed, but even in cases in which symptoms the most marked would have indicated to me the existence of serous apoplexy, before I had discovered my error, by the examination of bodies.

Different facts have convinced me that the practice of physicians, who prescribed emetics instead of bleeding in pretended serous apoplexies, was as fatal as the theory on which it was founded was erroneous, of which a judgment may

be formed from some facts I am about to report, and of which I could unfortunately too easily multiply the number.

Observation the First.

I was called, in the winter of 1785, to M. Debré, receiver-general, who had been seized with apoplexy. He was very large and very fat. Having been for some time subject to numbness, particularly during the night, he had been advised to lose blood, which he had not done. After dining one day very heartily (for he was a great eater), he suffered a severe attack of apoplexy. Dr. L., who was called, prescribed an emetic without effect. The patient did not vomit, and remained in a profound sleep. I was called to him, and wished to take blood from the jugular, or at least from the foot, but not being permitted, I requested a consultation, and MM. Bouvart and Borie were called in.

We decided that the patient should not only be bled, but that he should be bled several times. He was in fact bled five times in three days, and, in proportion as his vessels were emptied, he found himself in a better state. At first he recovered the movement of the fingers, of the hand, and of the foot of one side only; and successively the movements and sensibility of the limbs and trunk of the same side. His reason returned by remarkable degrees. He spoke, saw, and heard but on one side only, for all the left side continued for a long time paralysed. He continued to take aperients and purgatives in the form of an apozem. He passed to the use of the waters of Balaruc, and at length was enabled to walk; but his left arm remained paralysed for a considerable time, and his tongue was much embarrassed in its movements. A visit to the waters of Bourbonne almost entirely re-established him, and he died three or four years afterwards of a different disease, at a very advanced age.

Observation the Second.

In 1790, M. Boutin, treasurer of the marine, aged 60, and very fat, experienced a violent fit of apoplexy at the end of a repast, at which he had eaten largely, and drank wine, spirits, and coffee. An emetic, which had been given him in a very considerable dose before I arrived, produced no effect. I bled him twice in the foot in a very short time; speech returned, and the respiration became more free, but half his body remained paralytic. Blisters, purgative apozems, and ultimately the waters of Balaruc, produced the best effect. The patient insensibly recovered the movement of his limbs, of his inferior extremity first, and then of his arm, which I have observed in all cases recover the last.

 He

He entirely regained his health by the use of the mineral waters, and died three or four years afterwards on the revolutionary scaffold.

Observation the Third.

I was called, in 1794, during a severe winter, to M. de Fauveaux, aged 79, who had been attacked with apoplexy. Arriving promptly at his house, I found him senseless, but with a pulse strong and hard, having his respiration stertorous, and his visage red. Apoplexy was here unequivocally marked. The patient was taken as he left table after a repast, at which he had eaten heartily, and drank much wine and other liquors. I was at first inclined, in consideration of his great age, and the fear of indigestion, to administer an emetic; but reflecting on the imminent danger of the patient, and that he had a very short time to live, I thought that I ought not to lose any time by prescribing an emetic, which I had never known to succeed in a similar instance. I therefore bled him in the foot, to the great astonishment of the assistants.

This bleeding was so serviceable that he opened his eyes, moved the fingers of his left hand, and recovered more freedom of respiration.

Deglutition being now more free, I caused him to take an emetic, which produced the most beneficial evacuations. Speech returned to the patient, but he remained deaf, and paralytic of all the right side, from which he recovered in a short time by the use of blisters, of purgative apozems, and of the waters of Balaruc.

This patient was perfectly re-established, and died two years afterwards of a catarrhal putrid fever.

Observation the Fourth.

The citizen Gercy was conducted to prison with many other persons who were condemned to death by the revolutionary tribunal: he was acquitted.

A few days afterwards he was seized with apoplexy after a great repast. His respiration was embarrassed; his pulse contracted and small; his visage pale; his limbs very flexible, without motion; and he was generally insensible; yet deglutition remained in a slight degree. I gave him an emetic without effect.

I now did not hesitate to bleed him in the foot, in which I was so successful, that the patient passed almost instantaneously from a state of complete stupor and palsy to the most violent convulsions, which alarmed the attendants, who now considered him much worse than before, but I thought otherwise. On the contrary, I assured them that the citizen Gercy

Gercy was better, knowing that the convulsions which succeed to paralysis are always less formidable than the profound trance *(assoupissement)*, and that they may even be favorable. I learned this from reading, from clinical observations, and from experiments made on living animals; for I had found when we compressed their brain strongly, that they fell into a profound stupor, and that this stupor diminishing, was succeeded by convulsions.

I advised a second bleeding, which was performed. The convulsions ceased; the patient recovered a little recollection; he pronounced some ill articulated sounds, which in a short time became distinct; but he remained blind. Blisters and purgatives at first produced no effect.

I did not hesitate, however, to declare that I did not despair of the recovery of his sight, if he were conducted to Bourbonne, to Balaruc, or to Barége, persuaded as I was that the compression of the origin of the optic nerves, and of the nerves themselves, the blood-vessels of which I considered as gorged with blood, would be diminished in proportion as that fluid re-entered the circulation, and that vision would consequently be restored, which happened in fact a short time afterwards.

To these observations I could join many others which I have collected, and which would prove that emetics administered to some patients laboring under all the symptoms of serous apoplexy, and to others who were attacked immediately after a copious repast, had been without effect, and that recourse was necessarily had to bleeding; but I shall pass them in silence for the greater brevity.

An emetic administered in such cases is either useless or hurtful, and may even destroy the patient: in effect, if the compression of the nerves in the brain is strong, not only the limbs are paralysed, but the stomach also; then the emetic can exert no action, and is useless. But if any sensibility remains in the stomach, it produces vomiting; but the stomach and muscles of the lower belly, in contracting, force a reflux of blood towards the head; for in persons who vomit, all the parts of the head receive more blood than ordinary, of which we may judge from the redness of the face, the inflammation of the eyes, and bleedings from the nose, which often succeed vomiting: it is not, therefore, surprising that persons have perished under the operation of an emetic.

In this manner it often happens that women perish of apoplexy during parturition, of which I have seen several examples, and a remarkable one in a lady aged 19, who was attacked by a formidable fit, but which was dissipated by numerous bleedings prescribed by my colleague Hallé and myself, and by a successful delivery by citizen Marin.

It

It is an error to suppose that the apoplexy to which old men are so often subject is not sanguineous: the contrary has been proved by the examination of bodies of the most aged persons. Daubenton and Leroy, whose loss the Institute still deplores, died of this species of apoplexy.

The first remained paralysed five days in all his left side. The dissection of his body, at which I assisted with my colleague Cuvier, proved that this great man died from an extravasation of blood into the right ventricle. Daubenton had not been bled: his age; his visage more pale than red; his pulse, which was neither strong nor hard, did not appear to indicate blood-letting.

The citizen Leroy died also of sanguineous apoplexy, and very suddenly. My colleague Lassus and myself assisted at the opening of the body by the citizen Nicolle: we found a quantity of blood extravasated between the cranium and the brain, and much more in the ventricles of this viscus. We were informed that citizen Leroy had bled several times from the nose some days before his death; perhaps he might have escaped had he been bled at that time.

Thinness of apoplectic patients is no reason for not bleeding them; on the contrary, every thing proves that such patients have more blood than others, as Morgagni, Haller, and other physicians, have observed: thus I have bled, with the greatest advantage, patients extremely thin, under a severe apoplectic attack; of this the Marechal de Fitz-James, and the Abbé de Boismont, have furnished me with two examples well known in Paris. Could not every practitioner cite similar instances, if he had prescribed bleeding more boldly? But the apprehension that it may be injurious, sometimes in apoplexies which they suspect from unfaithful symptoms to be serous; sometimes under pretext of indigestion, which they gratuitously suppose to exist, or which they fear without reason; sometimes in relation to old age, which they falsely suppose to be destitute of blood; sometimes in consequence of the thinness of the patient; and, in fine, sometimes in consequence of his weakness, which is often only apparent: all these reasons, I say, prevent practitioners from bleeding in severe apoplexies; and very unfortunately for their patients, who for the most part die, or remain deprived of the use of all their limbs, or at least some of them, or blind, or deaf, or dumb, or with some other misfortune, which might have been prevented by a copious bleeding; which, by depleting the vessels, would have removed the compression on the origin of the nerves, if the congestion of blood was not too great; for it may be sufficient to render the largest bleedings inefficacious, which are now however the less indicated.

P.S. The

P. S. The following is another observation, proving that apoplexy may arise from excess of blood in the brain, although the face may be very pale.

M. Patricot, aged 68, of an ordinary stature, and rather large, who had been for some time deaf, and particularly of late in the right ear, on his return from the country, where he had passed some time, called at the Institute, to speak to one of its members, appearing to enjoy the best health.

He entered the house of Madame de la Rochefoucauld, with whom he lodged; he was called as customary to dinner, and was found in the middle of his chamber senseless, and almost dead. I was summoned instantly, and hastened without delay to his assistance, for the case required it, and M. Patricot was the ancient companion of my studies, and we were united in friendship. I was told that he had been found without sense and motion, particularly of the left side; he had recovered the movement of the right arm when I arrived, and he carried it continually to the right temporal region, as if he there experienced some pain; his respiration was short and sighing, his pulse very weak and undulating, his visage pale and cadaverous. M. Petitbeau, Madame de Rochefoucauld's surgeon, had been called, and had in vain attempted to give him an emetic; I ordered two blisters to his legs, and prescribed an irritating glyster, *(un lavement irritant,)* which the patient could not take. We caused him to inhale some volatile alkali, and endeavored without avail to make him swallow a few drops in some peppermint and orange-flower water; deglutition was intercepted.

Notwithstanding I was convinced that M. Patricot suffered under a most formidable attack of apoplexy, and although so many observations had apprised me, that paleness of the countenance, and weakness of the pulse, were no proofs of its not being sanguineous; and although different observations detailed in my memoirs as those of M. Boutin, of M. de Fourneau, and of others, had convinced me of the utility of bleeding, yet I dared not advise it in this instance; the body of M. Patricot being cold as ice, and his pulse very small and feeble, which had not been the situation of the other patients whom I have mentioned; they, on the contrary, preserved almost their natural heat.

I dared not prescribe bleeding, calling to mind this passage of Aretin's: "Cæterum si frigiditate multa, et torpore vena minime scindenda videatur, subluenda alvus est;"* but I

* De Morb. Anat. lib. i. cap. iv. p. 81.

observed,

observed, that if the heat should rise, and the pulse become more full, circumstances very likely to happen, I would advise the application of a dozen leeches to the neck. In effect, some time afterwards the coldness of the body disappeared, its heat returned, and the face of the patient, which had been extremely pale, became very red, crimsoned. Some leeches were placed on his neck, but they obtained little blood. I saw the patient early on the following morning in the last moments of his existence. He died an hour after my visit.

It may easily be imagined that I wished to examine the body, although I was persuaded that the apoplexy was sanguineous; it was necessary to be convinced by an observation, which was made by M. Boyer, my assistant anatomist to the Museum of Natural History, in presence of MM. Petitbeau, de Combettes, and Jean-Paul Martin.

No water was found in the cranium or in the brain, but a small quantity of blood was discovered in the ventricles; and a clot of black blood was lodged partly in an excavation of the right hemisphere, and partly in the right ventricle; all the other viscera were sound.

Remarks.—Wepfer also found a clot of blood nearly in the front of one of the hemispheres of the brain, in a woman seventy years of age, who, before the apoplectic attack which destroyed her, had experienced great defect in sight, and a loss of power in the movement of her tongue. The same author, one of the first who wrote on apoplexies from the observation of appearances after death, relates the history of an apoplectic, who, after experiencing a severe attack of gout, of which he was thought cured, was seized with a disease in his chest, which threatened a fatal phthisis pulmonalis; but he suddenly lost sense and motion; paleness of the face, and coldness of the extremities came on, whilst the pulse was strong, full, and frequent, and the respiration laborious. Wepfer did not venture to bleed this patient: "venam in debili tamen et brevi expiraturo tundere non ausus fui."

Nevertheless, *convulsions* supervened, with foaming at the mouth, and stertorous respiration; the body became cold, and the patient died. On opening the body, Wepfer found much blood between the membranes of the brain and in its ventricles: "omnes cruore adimpletos non quarto quidem excepto." The lateral ventricles appeared dilacerated about their base: "ac si nimio cruore distenti rimes egissent." Wepfer could not discover that any vessel of the brain had been ruptured, although more than two pounds of blood were effused.

To

To the Editor of the Medical and Physical Journal.

SIR,

SUCH of your readers as have seen Mr. Granger's paper in a northern quarterly Journal, will not think any reply from me to his captious and testy observations in your last Number at all necessary. Indeed, his own paper in that Journal, furnishes a complete answer to his paper in *your* Journal.

He accuses me of "misrepresentation, misquotation," &c. and even denies his own words, and says, "I make him say" that Ann Moore's illness was assumed, or, as he has it, *simulated.* His own words are, "The debility at the same time was so extreme as to occasion syncope. In the state of syncope, the pulse was 140, and indistinct." And then, in a note, his words are, "It is now admitted that the illness in a great degree was *simulated* for the purpose of exciting alarm." After quoting Mr. Granger's own words, it is hardly necessary to ask, whether I "make him say," as he states, in his reply to me, that "the pulse was 140, and indistinct?" But, really, the subject of Ann Moore is worn threadbare, and your readers will, I have no doubt, be glad to be released from any further discussions upon it, and on my part I shall not disappoint them. My object in my short notice of Mr. Granger's account of the second watch, was to reprehend what I conceived to be an unfair and even absurd statement, and an officious interference with, and anticipation of, the promised publication of Mr. Legh Richmond.

I am, Sir,

Your obedient Servant,

Derby, Sept. 10, 1813. JOHN WEBSTER.

For the Medical and Physical Journal.

ON GUN-SHOT WOUNDS.

THE following treatise is a translation from the surgical work of M. Richerand, a distinguished physiologist and professor of the Parisian school of medicine. To this translation some slight additions have been made; and one or two paragraphs on extracting instruments have been omitted, for the purpose of substituting an account of the instruments of M. Chevalier, which seem to be the most useful. The observations of M. Richerand merit peculiar attention, because, in addition to his acquaintance with the experience of the French army surgeons, it is evident he is not ignorant of the opinions of English surgeons, and above all those of Mr. John Hunter. To the latter circumstance, almost

almost as much as to the former, may, perhaps, be attributed the abandonment of those officious intermeddlings with the processes of nature, which characterised the practice of the French, so late as at the commencement of the French revolution. At that time, it would have appeared unpardonable negligence to leave a gun-shot wound without a certain number of *incisions* and *unbridlings*. "The first indication of the systematic surgeon," says M. Percy,* "is to *change the nature* of this wound, and convert it, as much as possible, into a bleeding wound." We shall see M. Richerand restricting the application of this practice, and pointing out, with considerable discrimination, though not in the most systematic manner, the cases in which *incisions* and *unbridlings* ought to be employed.

Gun-shot wounds are characterised by the disorganization of their surface. The extreme contusion observed in these wounds depends on the rapidity with which the body producing them is moved. The parts it touches are converted into a blackish slough, which caused the ancients to think that bodies, projected by gunpowder, were heated, and produced actual burns. Reason and experience have taught us, that, however great may be the rapidity of a projectile, it does not acquire any sensible heat during its course. The degree of heat which would render a ball capable of burning the human body, would be sufficient to melt the ball.

The wounds of fire arms do not bleed unless a large vessel should be opened: the neighbouring parts are livid, and the shock which accompanies is so sudden and violent, that the part struck experiences a kind of stupor, of which the whole animal economy frequently partakes.

The history and the treatment of gun-shot wounds was filled with the most pernicious errors, before Ambrose Paré had established the true theory of them. Cannon balls and bullets sometimes produce the most serious injuries, without hurting the skin. The soft parts have been reduced to a kind of jelly, and the bones themselves broken, although the integuments were uninjured; and this effect was long attributed to the displacement of the air by the projectiles. It was believed that this elastic fluid, when suddenly displaced by the shock of a cannon ball, was capable of pressing surrounding bodies with sufficient violence to tear their substance; but how can we conceive such a pressure in the midst of the free air? The effect observed ought to be produced constantly, when a ball passes near; but we may every

* Chirurgien d'Armée, p. 181, Ed. 1792.

day

day see bullets carry away the plume, the hat, the clothes, and even the hair of the soldier, without his experiencing any other inconvenience.

The oblique action of balls on the human body easily explains this extreme contusion, in which the skin is not affected. Sometimes also it depends on the weakness with which these balls strike, when, having expended the greater part of the motion impressed on them, they act only by virtue of their weight. They are then designated by the name of spent balls.

When the contusions of fire arms are pretty violent, the muscles and cellular membrane bruised, and reduced to a kind of jelly, similar to the lees of wine, the bone sometimes broken, the limb is often in a state of torpor, which inevitably brings on gangrene.

Gun-shot wounds may have only one, or they may have two outlets, the ball being arrested more or less deeply in the thickness of the part, or having passed quite through it. In the last case the two openings are diametrically opposite, in the greater number of instances: pretty often, however, the outlet does not exactly correspond with the entrance of the ball, its direction having been changed by the resistance opposed to it by a bone, a cartilage, a tendon, or even an aponeurosis. Thus a bullet, having pierced the skin of the leg near the internal ankle, has been known to glide between the tibia and the skin, ascend, and come out near the knee; sometimes having struck tbe forehead, it has issued at the temple, etc. Books relate a great many instances of these singular deviations. The directness of a ball's course will be in proportion to the rapidity of its motion, and also to the softness of the parts it passes through.

The wounds of musquet balls generally have the form of the body which produces them: they are round, square, or oblong; but when they have two openings, that at the entrance is always larger than that at the exit of the ball. Its edges are depressed; there is a depression near the entrance, while the parts are, as it were, raised, and make a projection towards the other opening. This difference takes place, because, at the moment the ball meets the limb, it strikes it with its full force, which it loses when it is buried in the thickness of the parts, in overcoming their resistance. The skin at the entrance is supported by the whole thickness of the limb; this point of support favors the solution of continuity, and prevents laceration: the contusion is also, for the same reasons, greater at the entrance of the ball, and when the swelling, which is always proportionate to the contusion, has taken place, the difference between the two openings is more

 strongly

strongly marked, for the entrance is more swelled than the outlet. These explanations are so well founded, that Le Dran, speaking of wounds of the head, remarks, there is no difference between the entrance and outlet, because the point of support is of the same nature.

The livid yellow color of the parts around a gun-shot wound, arises from the ecchymosis produced by the violence with which the blood is driven back, the slough preventing the escape of the humors. They swell the part, and much increase the importance of the wound. The part struck is benumbed, weighty, and in this state of torpor defends itself feebly against the entrance of liquids; the organic activity being almost extinguished, gangrene comes on, and makes the most rapid advances. This state of stupor and insensibility is especially fatal when the whole body experiences it; and this takes place in consequence of severe commotions, as when the body is struck by a large ball, or any other body of a certain bulk. In this state died the light horseman of whom Quesnay speaks: the state of stupefaction was such, that when they proposed amputation, the man answered "it was none of his business."

Jaundice is seen to occur suddenly from gun-shot wounds; also shiverings, faintings, and other nervous accidents, which caused the old surgeons to believe that gunpowder carried a secret poison into wounds; but the attrition which organs undergo, and the violent shock, of which the whole system partakes in a greater or less degree, are sufficient to explain the most mischievous consequences.

The complication arising from foreign bodies concealed in the wound, often exists in this species of injuries. These bodies are, either the ball itself, or portions of the clothes, which it has carried into the flesh. When the wound has but one opening, it very probably contains a foreign body: this is not certain, however; a number of cases are related, in which the ball, after making a deep wound of some inches, has been found in the shirt of the wounded man. In these cases, the linen is not even torn, but merely driven into the wound.

When this has two openings, we may conjecture that the ball has passed out; but even then, fragments of the clothes may remain in the passage, and this is the more easy, because these bodies being lighter, and pushed with a less force than the ball, are not capable of passing through an equal space.

Thus then the first indication in gun-shot wounds is to proceed to the search of foreign bodies, with which it may be complicated. Nothing can contra-indicate this search, but

but the danger of causing an unmanageable hemorrhage, by detaching the slough. A variety of ball forceps have been invented either to discover or extract the balls and other foreign substances, which the wound may contain. The finger is preferable to them all, when it is capable of reaching the foreign body; for the resistance made by bones and tendons, which lie in the course of the wound, may easily pass for some extraneous body.

The best forceps are, perhaps, those recommended by Mr. Chevalier. They are of two kinds; one for the extraction of balls, which is nearly like straight tooth forceps, except that the swell near the extremity is more considerable, for the purpose of receiving the ball. The other forceps are formed like common dressing forceps, but are of a greater size. A silver instrument has been invented, the form of which somewhat resembles that of an unblown tulip, the leaves of which are contrived to expand after the instrument has reached the ball, and thus having received it into their cavity, to close and retain it.*

It is useful, in searching for foreign bodies, with which gun-shot wounds may be complicated, to cause the patient to be placed in the situation he was when he received the wound. By means of this precaution, and carefully feeling the parts about the wound, Ambrose Paré discovered, in the Marshal Brisac, a ball placed between the shoulder blade and the spine, which had escaped the search of many surgeons, who had employed the probe alone.

It must not be forgotten that balls sometimes deviate from a straight course, in a very singular manner. In order that they should undergo these deviations, it is not necessary they should meet with bone, cartilage, or tendon. The mere difference of medium may produce a kind of refraction; for since water is able to turn a ball from a right line, the soft parts of the body will produce this effect in a greater degree, as their density is greater than that of a simple liquid.

These deviations of balls may cause us to believe that they have penetrated a cavity whose surface only they have passed over. Such, no doubt, was the case of a young drummer of the Swiss guards, who received a wound in the shoulder on the 10th of August. The ball had struck under the clavicle, and passed to the inferior angle of the scapula. Professor

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* This is not a modern invention. We find in Dionis the figure of an instrument of very similar construction, which he calls *Alphonsin* from the name of the inventor.

Boyer

Boyer extracted it by making a counter-opening. How could it pass through the upper part of the breast without wounding any of the important organs it must meet in its passage? Besides, it presented asperities, which evidently proved it had rubbed against osseous parts.

Sometimes, the nicest examination is not successful in discovering them; their passage is so tortuous, and they are so much refracted by the hard parts they meet in the way, that it is impossible to get at them. Then, by carefully examining the parts about the wound, and especially the part diametrically opposed to it, we perceive a foreign body through a greater or less thickness of parts, which it is necessary to incise, in order to extract it. We determine to make these counter openings the more readily, because a ball never can be extracted by the passage it had entered, without a considerable and painful traction on the parts. The new opening also facilitates the discharge of pus, and singularly shortens the cure, which is rendered tedious by the lodgment of this fluid, when the wound has only one outlet.

We must spare neither time nor pains for the extraction of foreign bodies, for their presence is a continual cause of irritation: they aggravate the accidents of wounds, and make them degenerate into fistulas. Sometimes, however, balls lie closely lodged in bones, during a long series of years, without danger and without pain. In some cases they pass a long way under the skin, making their way through the cellular membrane, without bringing on inflammation. The passage is then usually marked by the appearance of a reddish line in the skin. Balls have been seen to become a cause of irritation after they had been a long time quietly lodged in the body, bringing on a suppuration which removed them. We should not be too obstinate in searching for extraneous substances, because a very long and painful search might cause worse accidents than the matters lodged in the wound. When they are very difficult to find, we must abandon their expulsion to nature, or at least wait for symptoms to determine our course.

Is it always necessary to enlarge gun-shot wounds by incising their orifices? Some authors have laid down as a general rule the dilatation of these wounds. This operation, say they, besides that it singularly facilitates the search for foreign bodies, prevents the strangulation of the parts, when their inflammatory swelling comes on. More timid practitioners have gone so far as to forbid dilatation in all cases. According to them, these incisions increase the local disorder, facilitate gangrene, and are not without danger to the nerves,

the

the vessels, and the tendons, which may be implicated in them. It is true that this practice has been carried to excess; but the proscription of its abuse ought not to be extended to its use: it is, therefore, indispensable to determine the cases in which it may be necessary.

(To be continued.)

To the Editor of the Medical and Physical Journal.

SIR,

AS I have repeated my experiments several times on the Melksham water, and as I wish to make the account you honored me by inserting in your valuable Journal, as correct as possible, I beg leave to submit to you this short statement of the analysis. I also send you the results of experiments made for the purpose of establishing the real contents of the Cheltenham and Leamington waters, being authorised by a medical friend, who has devoted much time to the processes by which they were ascertained, the detail of which will shortly be presented by him to the public. You will observe that these analyses differ from such as have been lately given of these celebrated springs; and you will also see that the salts here enumerated are not incompatible with each other.

A quantity of the residuum which had been obtained from evaporating the Melksham water at the heat of 180°, was subjected to alcohol, and repeatedly washed in it until all the earthy muriates were dissolved and carried off. To this filtered solution both carbonate of ammonia and pure ammonia were added, and each produced a precipitation evincing the presence of both muriate of lime and muriate of magnesia.

Distilled water was then poured on the residuum to examine the nature of the sulphates, but nitrate of lime produced no precipitation in this filtered liquor, and pure ammonia did not detect any magnesia, consequently neither sulphate of soda nor sulphate of magnesia exist in this water.

The remaining residuum was dissolved in dilute nitric acid, and the fluid was filtered, leaving the sulphate of lime on the filter. In this fluid pure ammonia produced a slight precipitation showing magnesia; carbonate of ammonia separated the calcareous earth; and prussiate of potash evinced, by a strong blue color, the presence of iron.

The contents of this water, therefore, are muriate of soda, muriate of magnesia, muriate of lime, sulphate of lime, and the

the carbonates of lime, magnesia, and iron. I have put down the carbonate of magnesia, which, though questionable as an ingredient existing with muriate of lime, my experiments have uniformly detected. The muriate of lime being incompatible with the sulphates, except sulphate of lime, we could not expect it to exist in the Cheltenham waters, where Glauber's salt is a well-known ingredient.

The waters of Mr. Thompson's chalybeate and carbonate saline wells, as ascertained by the gentleman above mentioned, contain muriate of soda and sulphate of soda, no muriate of lime, no muriate of magnesia, and no sulphate of lime; a small portion of carbonate of soda, and, perhaps, a grain or two in a pint of sulphate of magnesia; the carbonates of lime, magnesia, and iron.

The water of Forty's old well contains muriate of soda, sulphate of soda, muriate of magnesia, sulphate of maghesia, and sulphate of lime; the three carbonates of lime, magnesia, and iron.

Leamington water contains muriate of soda, sulphate of soda, muriate of magnesia, a small quantity of sulphate of magnesia, and a very large portion of sulphate of lime.

From the above statement, it appears that the peculiarity of the Melksham water consists in its containing muriate of lime, and in its possessing at the same time a purgative quality, from which it may be entitled to some medical respect, and may establish some degree of reputation similar to that which has been so deservedly gained by the celebrated waters of Cheltenham and Leamington.

The Melksham water has been for some time used by the common people with much success for glandular, scrophulous, and scorbutic complaints; and already many very decided cases of those disorders, as well as such as proceed from indigestion and improper secretion of bile, have come to my knowledge. I may take the liberty of sending to you, at some future time, the detail of cases, as there seems sufficient reason to suspect that this variety of mineral water may have a correspondent effect on disease.

I have the honor to be, Sir,

Your most obedient Servant,

Bath, Oct. 10, 1813. G. S. GIBBES, M.D.

P. S.—I have not mentioned the quantities of each ingredient contained in the Melksham water, as I wait to compare the results of my experiments with those of Dr. Wilkinson, who is now engaged in the analysis.

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To the Editor of the Medical and Physical Journal.

SIR,

THE bill introduced into parliament last sessions by certain apothecaries, but withdrawn by them, from a suspicion, perfectly well grounded, that the wisdom of parliament would reject it, is again to be presented in order to be passed into a law. As this bill is intended to produce changes of considerable moment and interest both to the public and the profession, a short and candid review of it may be useful and acceptable.

First, it divides England and Wales into a certain number of districts, to be governed by medical committees, which district committees are to be entirely subordinate to a certain new organised body called the *London Superintending Committee;* and in order to insure this subjection, the individual members composing the district committees are to be *appointed* by the London superintending committee.

The provincial committees are not to be permitted to examine or grant licenses to practitioners of any description, nor even to visiting assistants: this power is to be vested exclusively in the London committee.

The duty of the provincial committees is to be limited to the humble task of examining and binding apprentices, and granting licences to journeymen shopmen and female midwives, and faithfully report their proceedings, to be reviewed by the London committee. The fair inference from this regulation is, that the inferior attainments and professional knowledge of the gentlemen in the country disqualify them from passing judgment on the qualifications of candidates.

The district committees are to make out annually, regular lists of every licensed practitioner, and affix them to their respective parish church doors. The reason given for this is, to enable the public to distinguish the regulars from the irregulars. On this clause it may be remarked that few gentlemen of known and acknowledged reputation will permit their names to be enrolled or published in this manner. Besides the utility of it is very questionable; for when the advertising quack is consulted, it is not because he is mistaken for a regular, but because he is known to be a nostrum vender, and the avowed enemy of the regular practitioner.

All the money, says the bill, arising from certificates, is to be at the uncontrolled disposal of the London committee. With this fund they may purchase lands, build halls, endow teachers, and doubtless *grant to themselves adequate salaries for their great care and trouble.*

Such is the new medical constitution by which the profession are now to be governed. There remains to be noticed a few minor clauses or enactments which respect apprentices, female midwives, and attendance fees.

Apprentices, on being bound, are to be previously examined on their knowledge of the Greek Testament; their indentures are to bear a stamp duty of 25*l.*; and they are to pay a further sum of 5*l.* 5*s.*, which last sum is *to be shared by the London and the district committees.* We pass over the pedantic folly of requiring from apprentices a knowledge in the Greek Testament, to observe that such an enactment will effectually preclude every gentleman of narrow income and practice from bringing up his sons to his profession, and will drive multitudes into Scotland for their medical education, in which country apprentices will not be subjected to pay such unreasonable fees to a business, the emoluments of which are very often of the most slender and precarious nature.

Every female practising or acting as a midwife without a certificate, the bill enacts, is to be fined in a blank sum of money. By this clause it follows, should any good motherly woman assist another in the pains of labor, and take any reward for her services, she is to be punished by a fine. No exceptions are provided from the necessity of the case, or the absence of a licensed practitioner. This humane enactment may be fitly classed with the clause exacting a sum of money from the poor youth who hires himself as an assistant, "whose learning is probably all his portion," before he can be allowed to earn his bread; but, thank God, this rapacious bill is not yet passed into a law.

Apothecaries, the bill says, may sue for and recover fees for visits and attendance. The power of suing for and recovering such charges has never yet been denied the profession by either courts or juries. The writer of this article has himself witnessed, in the court of Chester, actions brought for such fees, within this year or two, and liberal verdicts given. In truth, this bill will add considerably to the expence of a process; for it enacts that no apothecary shall be allowed to recover *any charges* unless he shall prove himself duly licensed under this act.

But the most dangerous privilege which the London committee assume to themselves, is the power given to them by this act of making and revoking bye-laws, rules, and constitutions, as they see fit.

Upon the whole, it appears that this bill is in the highest degree objectionable, is unjust in its principle, and oppressive in its operation; and will prove only beneficial to the members of the London superintending committee:—to

them

them it will be a source of profit, power, and patronage; to the rest of the profession insulting and degrading. Besides, the creation of this new body is unnecessary and uncalled for: when the public service or government require a test of the professional abilities of any individual, previous to employing him, there exist already a College of Physicians and Surgeons, whose competency and integrity have never yet been questioned.

With respect to private practice, mankind have and will always claim the right of applying to whom they please for relief from their maladies. The scholar and well-educated practitioner, by a faithful discharge of his professional duties, will not fail in due time to evince his superiority over the unprincipled practitioner, "and holding thus the noiseless tenor of his way," will reach at length an honorable independence. MEDICUS STOCKPORTENSIS.*

Sept. 15, 1813.

For the Medical and Physical Journal.

APPENDIX *to the* REPORT *of the* LONDON COMMITTEE *of* APOTHECARIES *and* SURGEON-APOTHECARIES.

(Continued from p. 315.)

No. I.

Copy of a Letter directed to Sir Francis Milman, Bart. President, &c. and to the Fellows of the Royal College of Physicians.

GENTLEMEN,

THE Committee appointed by a General Meeting of the Apothecaries of England and Wales, held at the Crown and Anchor Tavern, on Friday, the 20th of November, 1812, to carry into effect certain resolutions then agreed to, beg leave to transmit to you a copy of their report, with the resolutions subjoined, as approved of by that meeting.

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* We are compelled to remark in common candor, that the observations in this paper, by *Medicus Stockportensis,* apply *only* to the bill which has been relinquished, and can have no reference to that which is intended to be brought into parliament in the ensuing session, the clauses of which not yet being before the public, cannot possibly be the subject of animadversion. As far as we are enabled to judge, from the *resolutions* which the Committee have printed, and which this writer might have seen in our preceding number, as the bases of the new bill, his remarks will be totally inapplicable to that bill.—EDITOR.

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The Committee are of opinion that the management of the sick should be as much as possible under the superintendance of the physician; but as in a very great majority of cases the assistance and skill of the apothecary must be relied upon, no one should be allowed to practise as an apothecary without having previously submitted to a suitable examination.

The Committee suggest that there be a distinct privileged body established by the authority of parliament for such examination, and to superintend the general professional interests of apothecaries and surgeon-apothecaries throughout England and Wales, insuring by this means the better preservation of the public health. By the authority to be vested in the proposed superintending body, the apothecary will be required to be universally well qualified, and be thus rendered more worthy of public confidence. The Committee, therefore, submit, that he should possess a legal claim to moderate remuneration for his attendance and professional skill, under such modifications as may hereafter be judged proper.

The Committee are satisfied that any measures which may be considered as calculated to promote their views, must be confirmed by an act of the legislature, which will, unavoidably, be attended with considerable expence. They have, consequently, made it a primary object, and provided such a fund, as will, in their estimation, be adequate to that purpose.

The Committee are desirous to obtain the sanction and concurrence of the legally-constituted bodies of the profession; and they wish it to be distinctly understood, that they are extremely anxious that the regulations to be proposed shall, in no degree, interfere with their established privileges.

They have, therefore, determined to address the Royal College of Surgeons, and the Society of Apothecaries, at the same time with yourselves; and trust that they shall receive your countenance and support in a petition to parliament for the protection and regulation of the practice of the apothecary.

(Signed by desire of the Committee)

Bloomsbury-square, G. M. Burrows, Chairman.

Dec. 11, 1813.

Note.—A similar letter was addressed to Thompson Forster, Esq. Master, &c. and to the Governors and Court of Assistants of the Royal College of Surgeons: and another with the latter paragraph omitted, and the following paragraph

graph substituted, to the Master, Wardens, and Court of Assistants, of the Society of Apothecaries.

"They have, therefore, determined to address the Royal College of Physicians and Surgeons at the same time with yourselves.

But as the major part of the members of your society have the same general interests as other apothecaries, and can appreciate more correctly the extent of their grievances, the Committee trust their application to you will be favorably received and countenanced.

With such approbation they will be enabled more confidently to call upon the Royal College of Physicians and Surgeons, to unite in a petition to parliament for the protection and regulation of the practice of the apothecary.

(Signed by the desire of the Committee)

Bloomsbury square, G. M. Burrows, Chairman."

Dec. 11, 1812.

No. II.

(Copy.) *Bloomsbury-square,*

Dec. 25, 1812.

SIR,

As Chairman of the Committee elected by a General Meeting of the Apothecaries of England and Wales, I was instructed to direct the report and resolutions of that meeting, with a letter from the Committee, to the President and Fellows of the Royal College of Physicians.

Conformably with those instructions, I had the honor to send to you, as the president, the printed report of the meeting, and the letter of the Committee, signed by me as chairman, on the 11th instant, for you, officially, to submit to the fellows.

It has been intimated, that my letter, with the report, may be considered by the College a private communication to you; but though this appears to me improbable, yet, to prevent so erroneous an impression, I take the liberty of addressing you again as president, to request that you will lay the said letter, with the report, before the Royal College of Physicians at their next meeting, and that you will condescend to give orders that the Committee be favored with an early answer.

I have the honor to be, Sir,

Your obedient humble Servant,

G. M. Burrows, Chairman.

To Sir Francis Milman, Bart.

No.

No. III.

SIR, *Brook-street, Dec. 25, 1812.*

I had the honor of your letter of the 11th, and I have this moment received your favor of this day's date. I understood your first letter to have been only a circular form, of which you had addressed copies to a considerable number of my brethren, as you expressed a determination to address the Royal College of Surgeons and the Society of the Apothecaries at the same time with the College of Physicians. I waited for the execution of this declared purpose, and expected to receive your intended formal application, with your commands to lay it before the College, which I would have obeyed with great pleasure on the 22d of this month, the usual day of our meeting. No such document having reached me, I thought it possible the Committee might have altered the resolution they had formed; and not being particularly desired to do so, I did not deem myself justified in laying your first letter before the gentlemen assembled on the 22d in Warwick-lane. Should it continue to be the wish of your Committee, I will take the first opportunity of a meeting of the College to lay your letters before them.

I have the honor to be, Sir,

Your most obedient humble Servant,

To G. M. Burrows, Esq. F. MILMAN.
Bloomsbury-square.

No. IV. *Bloomsbury-square,*

SIR, *Dec. 26, 1812.*

Accept my acknowledgments for the distinguished attention with which you honored my letter of yesterday, by your answer of the same date.

The letter of the 11th instant, which I subscribed and sent by desire of the Committee of Apothecaries, was directed to the President and Fellows of the Royal College of Physicians. The Committee, therefore, could not conceive it essential to request a letter to be laid before your learned colleagues, which was expressly addressed to them as well as to you, sir, their president; and which they consequently imagined would be presented, as a matter of course, at the meeting of the College on the 22d of December.

The report was sent as a mark of respect to every member of the executives of the medical bodies to whom the Committee were directed to apply, by the fifth resolution of the General Meeting of Apothecaries, held November 20th; but the letter of the Committee was intended as the specific application, and was particular, and not general or circular.

May I take the liberty of soliciting that you will be pleased to inform me when the meeting of the College will take place, to which you propose submitting the letter of the 11th instant, that I may communicate your reply to the Committee?

I have the honor to be, Sir,

Your obedient humble Servant,

G. M. BURROWS, Chairman.

To Sir Francis Milman, Bart.

No. V. *Bloomsbury-Square, Dec. 26, 1812.*

SIR,

The letter from the Committee of Apothecaries subscribed by me, dated December 11th, and addressed to you as the Master, and to the Governors and Court of the Royal College of Surgeons, I request you will lay before the College, at their first meeting, if that be not already done; and I further take the liberty to express my hope, that you will return an answer as soon as the necessary forms admit.

I have the honor to be, Sir,

Your obedient humble Servant,

G. M. BURROWS, Chairman.

To Thompson Forster, Esq.
Master of the Royal College of Surgeons.

No. VI.

Southampton Street, Bloomsbury, December 28, 1812.

Mr. Forster's respects to Mr. Burrows, and begs leave to mention that he laid his letter from the Committee of Apothecaries before the Court of Examiners on the 18th instant; that Court not being competent to the taking it into consideration, it was referred over to the first General Court of Assistants of the College of Surgeons.

To G. M. Burrows, Esq.
Chairman of the Committee of Apothecaries.

No. VII. *Bloomsbury Square, Jan. 1, 1813.*

SIR,

The Committee of the Apothecaries of England and Wales have been made acquainted with the correspondence which has taken place with you, relative to their letter of the 11th of December, directed to the President and Fellows of the Royal College of Physicians.

I have the honor to inform you, that they are extremely concerned to find it was not presented at their meeting on the 22nd, agreeably to their intentions. The Committee therefore make it their particular request, that it may be submitted to the College.

As

As I have not yet been favored with an answer to the question with which I closed my letter of the 26th, the Committee beg leave most respectfully to suggest their wish, that you may think it right to call a meeting of the College at an early period, for the purpose of taking their letter of the 11th into consideration.

The Committee, with great deference, venture this proposal; but further delay will be attended with material inconvenience, as it may prevent the passing of a Bill through Parliament during the present Session.

I have the honor to be, Sir,
Your obedient humble Servant,
To Sir F. Milman, Bart. G. M. Burrows, Chairman.

(To be continued.)

COLLECTANEA MEDICA,

CONSISTING OF

ANECDOTES, FACTS, EXTRACTS, ILLUSTRATIONS, QUERIES, SUGGESTIONS, &c.

RELATING TO THE

History or the Art of Medicine, and the Auxiliary Sciences.

Additional Remarks on the State in which Alcohol exists in fermented Liquors. By William Thomas Brande, Esq. F.R.S. From the Philosophical Transactions. Read before the Royal Society, December 17, 1812.

THE experiments and observations contained in this paper, are intended as supplementary to a communication on the same subject, which the Royal Society has done me the honor to insert in the Philosophical Transactions for the year 1811.*

On that occasion, I endeavored to refute the commonly received opinion respecting the *production* of alcohol during the distillation of fermented liquors, by showing, that the results of the process are not affected by a variation of temperature equal to twenty degrees of Fahrenheit's scale; that is, that a similar quantity of alcohol is afforded by distilling wine at 180° and at 200°.

I also conceived, that any new arrangement of the ultimate elements of the wine, which could have given rise to the formation of alcohol, would have been attended with

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* P. 337.

other

other symptoms of decomposition; that carbon would have been deposited, or carbonic acid evolved, which in the experiments alluded to, was not the case. Upon such grounds I ventured to conclude, that the relative quantity of alcohol in wines, might be estimated by submitting them to a careful distillation, and by ascertaining the specific gravity of the distilled liquor with the precautions which I have formerly described.

This conclusion may be objected to, by supposing that the lowest temperature, at which the distillations were performed, was sufficient for the formation of alcohol from the elements existing in the wine; but it is not easy to conceive how this should happen, without some of those other changes which I have just noticed.

It has been stated, in my former paper, that the separation of alcohol from wine, by the addition of subcarbonate of potash, is prevented by the combination of the alkaline salt with the coloring-extractive, and acid contained in the liquor. I have also shortly noticed some unsuccessful attempts to separate these substances by other means than distillation.

In prosecuting the inquiry, this difficulty has been surmounted, and I shall proceed to show, that alcohol may be separated from wine without the intervention of heat, and that the proportion thus afforded is equal to that yielded by distillation.

When the acetate,* or subacetate† of lead, or the subnitrate of tin‡ are added to wine, a dense insoluble precipitate is quickly formed, consisting of a combination of the metallic oxide, with the acid and coloring-extractive matter of the wine; and when this is separated by filtration, a colorless fluid is obtained, containing alcohol, water, and a portion of the acid of the metallic salt, provided the latter has not been added in excess, in which case a part remains undecomposed.

The acetate of lead and the subnitrate of tin produce the desired effect of separating the coloring and acid matters, in the greater number of instances, but they are less rapid and perfect in their action, and not so generally applicable as the

* Sugar of lead.

† Formed by boiling two parts of sugar of lead with one of finely powdered litharge in six parts of water. The solution should be preserved in well-closed phials, as it is rapidly decomposed by attracting carbonic acid from the atmosphere. Even while hot, a portion of carbonate of lead is formed in it.

‡ Prepared by dissolving protoxide of tin in cold dilute nitric acid.

the subacetate of lead,* which is the substance that I commonly employed.

The following experiment was made with a view to ascertain the effect of this salt.

Twenty measures of alcohol, specific gravity ,82500, were mixed with eighty measures of distilled water colored with logwood, and rendered slightly acid by supertartrate of potash. Four measures of a concentrated solution of the subacetate of lead were added to this mixture, and the whole poured upon a filter. A precipitate was thus collected of a deep purple color, which appeared to consist of oxide of lead combined with tartaric acid and the coloring-extractive matter.

The filtered liquor was perfectly transparent and colorless, and afforded, on the addition of subcarbonate of potash, 19,5 measures of alcohol.†

Finding that the separation of alcohol by subcarbonate of potash from mixtures of spirit and water, was nearly complete, and that coloring-extractive matter, and tartaric acid might be removed from such mixtures by the subacetate of lead, I proceeded to examine wine by such modes of analysis.

The following results were obtained by these, and other comparative experiments.

1. One part by measure of a concentrated solution of subacetate of lead, was added to eight measures of common

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* The effect of this salt upon coloring matter, was first pointed out to me by Mr. E. M. Noble, of Chelsea.

† Pure subcarbonate of potash, obtained by igniting the carbonate, was employed in these experiments. I found that about 19,5 parts of alcohol were separated in the course of four hours, by the addition of 50 parts of the subcarbonate to a mixture of 20 parts of alcohol by measure with 80 of distilled water, and that no further separation took place. The alcohol is always slightly alkaline, probably from containing a small portion of the solution of the subcarbonate, or of pure soda; but as this did not interfere with the object of the experiment, it was not particularly attended to.

When the subcarbonate was added to a mixture of four parts by measure of alcohol with 96 of water, no separation was effected.—A mixture containing 8 per cent. of alcohol afforded about 7 parts—one containing 16 per cent. about 15,5, and where the proportion of alcohol exceeded 16 per cent. the quantity, indicated by the action of the subcarbonate, was always within 0,5 per cent. of the real proportion contained in the mixture. So that in the examination of wines containing less than 12 per cent. of alcohol, the method described in the text is somewhat exceptionable. The above experiments were made in glass tubes varying in diameter from 0,5 inch to 2 inches, and accurately graduated into 100 parts.

port

port wine: the mixture having been agitated for a few minutes, was poured upon a filter. The filtrated liquor was perfectly colorless, and the addition of dry subcarbonate of potash effected a rapid separation of alcohol.*

100 measures of the wine thus treated, afforded 22,5 measures of alcohol.

2. Eight ounces of the wine employed in the last experiment, were distilled in glass vessels, as described in my former paper. The specific gravity of the distilled liquor at the temperature of 60° was 0,97530, which indicates 22,30 per cent. by measure of alcohol of the specific gravity of ,8250.

3. Eight ounces of the same wine were introduced into a retort placed in a sand heat, and the process of distillation was stopped when six ounces had passed over into the receiver. After the vessels were completely cooled, the portion in the receiver was added to the residuum in the retort. The specific gravity of this mixture (ascertained with proper precautions) was ,9884, that of the original wine = 0,9883.†

When care was taken to prevent the escape of vapor, no change of specific gravity was produced in the wine by three repetitions of the above process.

Similar experiments were repeated upon Madeira, Sherry, Claret, and Vin de grave, wines differing in the relative proportions of alcohol, coloring matter, and acid which they contain, and the results were as decisive; so that I conceive it is amply proved, by experimental evidence, that no alcohol is *formed* during the distillation of wines, and that the whole quantity found, after distillation, pre-existed in the fermented liquor.

It has been frequently asserted, that a mixture of alcohol and water, in the proportions I have stated them to exist in wine, would be much more effectual in producing intoxication, and the general bad effects of spirituous liquors, than a similar quantity of the wine itself. But this is true to a very limited extent only: when brandy is added to water, it is some time before the two liquids perfectly combine, and with alcohol this is more remarkably the case, and these mixtures are warmer to the taste, and more heating, if taken in this state of imperfect union, than when sufficient time has been allowed for their perfect mutual penetration.

I have also ascertained that distilled port wine tastes

* When any excess of the subacetate had been employed, a portion of carbonate of lead was thrown down; but this did not interfere with the subsequent separation of the alcohol.

† This experiment was suggested in the Edinburgh Review for November, 1811.

 stronger,

stronger, and is more heating than the wine in its original state, and that these qualities are impaired, and the wine reduced nearly to its original flavor, by the addition of its acid and extractive matter. With claret, and some other wines, containing less alcohol and more acid than port, these circumstances are more readily perceived; and lastly, if the residuum afforded by the distillation of 100 parts of port wine, be added to 22 parts of alcohol and 88 of water (in a state of perfect combination), the mixture is precisely analogous in its intoxicating effects to port wine of an equal strength.

In the table annexed to my former paper, it appears that the average quantity of alcohol contained in port wine amounts to 23,48 per cent.; but two of the wines there alluded to are stronger than any I have since met with, and were, at that time, sent to me as "remarkably strong and old port." I have lately examined a number of specimens of the better kinds of port wine in common use, and the results of these experiments lead me to place the average strength at 22 per cent. of alcohol by measure.

A port wine procured for me by Dr. Baillie, and to which no brandy had been added, afforded 21,40 per cent. of alcohol: another specimen of a similar description, put into my hands by an Oporto merchant, contained only 19 per cent.; it is the weakest port wine I have met with.

The other results given in the table, agree perfectly with those of subsequent and more extended experiments.

Observations on Radiant Heat. By F. DELAROCHE, M.D.*

I propose in this memoir to state several propositions which appear to me capable of throwing some light on the theory of radiant heat; and which, I think, I have established by decisive experiments. These experiments, indeed, were made with sufficient care to prevent any doubts about their exactness; but I may be deceived in the conclusions that I deduce from them. In that case I shall readily acknowledge my error; nor shall I think that I have lost my labor if I draw, upon so interesting a subject, the attention of some philosopher more fortunate than myself, or better situated for examining it with accuracy.

First Proposition.—*Invisible Radiant Heat may in some circumstances pass directly through Glass.*

Different philosophers, and particularly Mr. Leslie, conceive that they have proved the falsehood of this proposition; but the experiments of Professor Prevost, of Geneva, have lately established its truth in an incontestable manner. He

* Abridged from the *Journal de Physique*, vol. lxxv. p. 201.

obtained

obtained his result by separating the immediate effects of transmitted heat from those produced by the heating of the glass, by a process equally simple and ingenious; namely, by employing moveable screens of glass, which he renewed continually, and of course did not give them time to become heated. I have myself, since I became acquainted with the memoir of M. Prevost, made a great many experiments, which appear to me to prove the same thing. The nature of these will be stated in support of the second proposition.

Second Proposition.—*The Quantity of Radiant Heat which passes directly through Glass is so much greater relative to the whole Heat emitted in the same Direction, as the Temperature of the Source of Heat is more elevated.*

Dr. Delaroche shows, in the first place, by some ingenious experiments, that a thermometer (of the temperature of the surrounding atmosphere) exposed to the action of radiant heat rises in a given time, cæteris paribus, a quantity proportional to the rays which it receives. If a certain number of rays make it rise 1 degree, double the number will make it rise 2 degrees, triple the number 3 degrees, and so on. This being established, he placed two parabolic metallic mirrors at the distance of rather more than three feet from each other. In the focus of one mirror he placed a thermometer; and in the focus of the other a hot body, gradually increasing in temperature. The thermometer was allowed to rise to its maximum without any screen. Then a screen of transparent glass was interposed, and the experiment repeated. Lastly, a screen of blackened glass was interposed, and the experiment repeated a third time. By the blackened screen all the radiant heat would be intercepted, and the effect on the thermometer would be owing to the rise in the temperature of the screen. Hence the rise of the thermometer, when the blackened screen was used, subtracted from the same rise when the transparent screen was used, leaves the effect produced by the radiant heat passing through the glass. The following table shows the ratio between the rays passing through the clear glass, and the rays acting on the thermometer, when no screen was interposed, at different temperatures:—

Temperature of the Hot Body in the Focus.	Rays transmitted through the Glass Screen.	Total Rays.
357°	10	263
655	10	139
800	10	75
1760	10	34
Argand's lamp without its chimney	10	29
Ditto, with glass chimney	10	18

Third

Third Proposition.—*The Calorific Rays which have already passed through a Screen of Glass, experience, in passing through a second Glass Screen of a similar nature, a much smaller Diminution of their Intensity than they did in passing through the first Screen.*

This proposition was proved by interposing, first one glass screen, and then two, and observing the difference in the effect. The experiments appear conclusive.

Fourth Proposition.—*The Rays emitted by a hot Body differ from each other in their faculty to pass through Glass.*

This proposition is an obvious consequence from the third, which can hardly leave a doubt that the calorific rays (like those of light) are of different kinds.

Fifth Proposition.—*A thick Glass, though as much or more permeable to Light than a thin Glass of worse Quality, allows a much smaller Quantity of Radiant Heat to pass. The Difference is so much the less as the Temperature of the radiating Source is more elevated.*

This proposition is proved by experiments made with glasses of different thickness employed as a screen; and its truth is sufficiently established, if these experiments be accurate. This curious fact, that radiating heat becomes more and more capable of penetrating glass, as the temperature increases, till at a certain temperature the rays become luminous, leads to the notion that heat is nothing else than a modification of light, or that the two substances are capable of passing into each other.

Sixth Proposition.—*The quantity of Heat which a hot Body yields in a given time by Radiation to a cold Body situated at a distance, increases, cæteris paribus, in a greater Ratio than the excess of Temperature of the first Body above the second.*

This proposition being at variance with the opinion of Mr. Leslie, and of several other philosophers, Dr. Delaroche thought it necessary to establish it by a great variety of experiments. These experiments leave no doubt of the fact; though they are not sufficient to enable us to deduce the rate at which the increase takes place.

I must acknowledge that this proposition appears to me somewhat puzzling. One is at first sight disposed to account for it by the inaccuracy of the thermometer as a measure of heat; but Dr. Delaroche, aware of such an objection, has remarked, that if we adopt Mr. Dalton's opinion with respect to the thermometer, so far from removing the apparent anomaly, it would only serve to make it greater.

Memoir

Memoir on the Determination of the Specific Heat of the different Gases. By MM. F. Delaroche, M.D. and J. E. Berard.*

The subject proposed by the Institute as a prize at the meeting of the 7th January, 1811, namely, the determination of the specific heat of the gases, had previously attracted the attention of different philosophers, some of whom have treated of it in detail; yet so little progress has been made in the investigation, though the question proposed be sufficiently simple, that we are almost as far from being able to answer it with precision, as we were before it became the subject of investigation.

Crawford, as far as we know, is the first person who began the investigation. He published the result of his researches in 1788. At that time a great number of experiments had been made upon the specific heat of bodies in general. MM. Lavoisier and de Laplace had already published the result of their experiments on that subject, and had given a description of their calorimeter of ice; yet Dr. Crawford preferred the method of mixtures of water, or other bodies whose specific heat was considered as known. After many unsuccessful attempts, he thought that he succeeded by the following method. He procured two copper vessels, very thin, and of the same shape, size, and weight. He filled one of them with the gas that he wished to examine, and made a vacuum in the other. He then heated both in boiling water, and plunged both suddenly into cylinders containing a small quantity of cold water, but sufficient to cover them. He subtracted the heat communicated to this water by the exhausted vessel from that communicated by the vessel full of gas, and considered the remainder as the effect produced by the gas, or as its specific heat. He had taken great precautions to ensure the accuracy of his results; but it is quite evident, from the smallness of the change, that no confidence could be placed in them. The rise in the temperature of the water never exceeded 0·4° Fahrenheit. The following is the table of his results:—

Specific heat of Water	1·000
Atmospheric air	1·790
Oxygen	4·749

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* Translated from the Annales de Chimie for January, 1813, vol. lxxxv. p. 72. This memoir gained the prize proposed by the Institute, and deserves particular attention. It overturns the theory of animal heat advanced by Crawford, and Lavoisier's theory of combustion.

Specific

Specific heat of	Azote	0·793
	Carbonic acid	1·045
	Hydrogen	21·400

Before the work of Crawford appeared, Lavoisier and Laplace had made some experiments, which were not published till long after. They had employed their calorimeter of ice, through which they passed a current of gas contained in a serpentine, which enveloped on all sides the ice of the interior chamber. A thermometer placed at each extremity of the serpentine enabled them to observe the temperature of the gas when it entered and came out of the calorimeter. The gas was heated by passing through a serpentine surrounded with boiling water, before it entered into the calorimeter. These experiments, though susceptible of much greater precision than those of Crawford, were not free from very material imperfections. The method employed by these philosophers to take the temperature of the gases at its entrance into the calorimeter was insufficient, since the gas, in passing through the exterior coating of ice, would lose a portion of the heat which the thermometer had indicated, without contributing by that to melt the ice in the interior chamber. On the other hand, they do not say that they took any precautions to dry the gases upon which they made their experiments. These gases, charged with the humidity which the contact of water in the gazometers would necessarily communicate, no doubt deposited the whole of it when they passed through the calorimeter; but we know that vapor, when it condenses, gives out a great deal of heat. It is proper, however, to remark, that the temperature at which these experiments were made, being probably but little elevated above the freezing point, the quantity of vapor mixed with the gas would not be considerable. Lavoisier and de Laplace only subjected to these experiments oxygen and atmospherical air. They found for the specific heat of the first (that of water being 1·00) 0·65, and for the second 0·33; but Lavoisier acknowledges that the accuracy of these results cannot be entirely depended on.

Since that time, various attempts have been made to appreciate, by indirect means, the specific heat of some gases. Mr. Leslie has employed, in order to compare the specific heat of hydrogen and atmospheric air, a process founded on the following considerations. When a large receiver is partly exhausted of air, if air be allowed to enter into it, the dilated air which it contained will condense, and its temperature will be increased by a constant quantity, whatever gas enter into it; but the entering gas will absorb a part of this excess of

heat, and the mixture will have a mean temperature between that of the entering gas and that which the air would have acquired if it had not been obliged to part with any of its heat. Now it is evident that this mean temperature will be so much the lower the greater the specific heat is of the gas which enters. The experiments made by Mr. Leslie have led him to conclude, that equal volumes of hydrogen and atmospherical air have the same specific heat.

The principle upon which this ingenious process is founded is not perfectly just; since it appears from the experiments of Gay-Lussac,* that part of the heat developed in this case comes from the gas that enters into the receiver. It appears, likewise, that some unknown circumstance has misled Mr. Leslie respecting the result of his experiments: for analogous experiments, made with the greatest care, have given different results to Gay-Lussac; and he did not observe that equality of effect which takes place, according to Mr. Leslie, when atmospherical air and hydrogen gas are made to enter into an exhausted receiver. Gay-Lussac, in these experiments, made use of two similar balloons, communicating with each other by a pipe furnished with a stop-cock. He made a vacuum in the one, and filled the other successively with different dried gases. In the centre of each of these balloons was a thermometer of spirit of wine. When the stop-cock was turned, the gas rushed from the full vessel into the empty one. He had taken precautions to render the velocity of the current equal in all cases. The thermometer of the first balloon sunk, and that of the second rose the same number of degrees; but that number varied according to the nature and density of the gases employed. Gay-Lussac, thinking that the specific heat of the gases subjected to these experiments was proportional to the rising and falling of the thermometers, thought himself entitled to conclude that the specific heat of equal volumes of the different gases was inversely as their specific gravity, and of the same gas directly as its density; but he only gave this opinion as a probable conjecture, without affirming any thing respecting its justness. In fact, the phenomena which take place in such cases are very complicated, and it is almost impossible to distinguish what depends upon the different conducting power of the gases from what depends upon their specific heat.

M. Gay-Lussac has discovered this himself, in the new memoir which he has lately published on the subject,* and in which he came to different results. He employed in these last experiments a very simple and ingenious method to de-

* Mem. d'Arcueil, i. 180. † Ann. de Chim. lxxxi. 98.

termine the specific heat of the gases. It consists in passing to the centre of a small reservoir, containing a thermometer, a current of two different gases, the one hot, the other cold. Knowing the temperature of the two gases before their mixture, and that of the mixture, it was easy to infer the ratio between their respective specific heats. This process, besides that it does not give us the ratio between the specific heat of the gases and that of water, is attended with another inconvenience. When only small quantities of gas are employed, a great part of their heat is communicated to the vessels in which the mixture is made, which may lead to erroneous conclusions. Accordingly, as he made his experiments at first on the gases that can be most conveniently procured, such as air, hydrogen, carbonic acid, &c. which, as will be seen afterwards, do not differ much in their capacity for heat, he was induced to believe that the same volumes of all the gases had the same capacity. However, he afterwards published a note,* from which we see that he had brought his process to perfection by operating upon large quantities of gas. By that method he ascertained that hydrogen and carbonic acid have different specific heats, and the numbers which he assigns approach to those which we have ourselves obtained. This induces us to remark to the commissioners, that a first memoir having for its motto *Tectus magis æstuat ignis*, which contains our most important results, was deposited in the hands of the secretary of the Institute on the 3d of February, 1812, more than five months before that note of Gay-Lussac was published in the Annales de Chimie.

Among the attempts made to determine the specific heat of the gases, we ought to reckon the table drawn up by Mr. Dalton from principles purely theoretical, founded on this hypothesis, that the quantities of heat belonging to the ultimate particles of all elastic fluids ought to be the same under the same pressure and at the same temperature. His table is as follows:—

Hydrogen gas	9·382	Nitrous gas	0·777
Azotic gas	1·866	Oxide of carbon	0·777
Atmospheric air	1·759	Vapor of alcohol	0·586
Ammonia	1·555	Sulphureted hydrogen	0·583
Olefiant gas	1·555	Nitrous oxide gas	0·549
Oxygen	1·333	Vapor of nitric acid	0·491
Carbureted hydrogen	1·333	Carbonic acid	0·491
Aqueous vapor	1·[illegible]66	Muriatic acid	0·424
Vapor of ether	0·848		

* Ann. de Chim. lxxxiii. 106.

Such

Such are the results of the investigation of this subject hitherto made. By comparing them together, it is easy to see how far they differ from one another. Unless we deceive ourselves respecting the justice of those results which we ourselves have obtained, it will be seen, by what follows, how far they all deviate from the truth.

CRITICAL ANALYSIS
OF RECENT PUBLICATIONS
IN THE
DIFFERENT BRANCHES OF PHYSIC, SURGERY, AND MEDICAL PHILOSOPHY.

Appendix to Observations on the Contracted Intestinum Rectum: containing some additional Facts relative to that Complaint; with several Cases, and two Engravings. By W. White, Member of the Royal College of Surgeons, London, and one of the Surgeons to the City Infirmary and Dispensary, Bath. 8vo. Bath, 1813. pp. 53.

SOME time ago we gave an analysis of Mr. White's treatise on Contracted Intestinum Rectum. The ingenuity and practical utility of that treatise, we endeavored to impress on our readers. The subject, always of importance, had been brought more under public observation than heretofore, by the publication of Mr. Copeland; the immediate inducement, as it then seemed, to Mr. White's pursuing the inquiry. Since that period many cases have fallen under the care of Mr. White, nine of which are here related.

There are two points in this little volume principally to be looked to: one of these is a practical fact of some importance; the other assumes more the character of an hypothesis, though it is not directly hypothetical.

A marked trait in the contracted rectum, and constituting a prominent feature in the diagnosis, is that of the fæces being found to be lessened in their diameter. The occasional deviation from this, in advanced stages of the disease, when fæces of a natural size, at times, pass, Mr. White notices, with a view to prevent the practitioner mistaking the cases when this circumstance does arise; and as it appears mechanically contradictory, he enters into the following explanation of it.

"If the stricture should happen to be low in the rectum as not to allow room for the accumulation of fæces, it must appear evident that

 they

they will be found uniformly small in diameter, in proportion to the degree of stricture, while they continue to be discharged in a figured state. And also, when the stricture happens to be high up the rectum, so long as the gut below the stricture retains its natural expulsive power, an accumulation will be prevented, and the diminished size of the fæces will continue. But as the disorder increases, the inferior portion of the intestine gradually loses that power; so that when the contraction becomes considerable, only a small quantity of fæces pass at a time through the stricture, and not being sufficient to stimulate the lower part of the rectum, an accumulation goes on from time to time, until at length it becomes difficult to remove; when on these occasions fæces of a natural size have sometimes been discharged."

It has now some time been the fashion, or rather, perhaps, has now become an admitted fact, that strictures in the urethra, by some, yet unexplained, sympathy, produce morbid actions in distant parts of the system, of a character, *à priori*, not at all referable to such a cause. Mr. White does not doubt but strictures of the rectum may excite derangement in remote parts of the intestinal canal, or in organs that are associated with it in the performance of its natural functions. The *liver* is the organ to which our author looks as most likely to undergo this sympathetic derangement. In two patients a tuberculated state of the liver was believed to be the consequence of stricture in the intestine; and as the opinion is in a measure new, we shall lay before our readers the whole of the third case, which goes, in the author's opinion, particularly to this point. In this the biliary secretion was disturbed, and the alvine evacuations were always of a light clay color, except when the patient was taking calomel.

"Case 3.—Feb. 1812, E. Morgan, an unmarried woman, sixty-three years of age, complained of having been subject to pains about the os sacrum shooting down the hips, between four and five years. She had been always of a costive habit of body, seldom having any evacuation for four or five days, and not then without the aid of a strong purgative medicine. About a year ago she was attacked with a sudden hæmorrhage, which she supposed to have been a return of the catamenia in a most extraordinary and violent manner; but on the hæmorrhage recurring shortly after, she was convinced the discharge proceeded from the rectum; ever since which she has had frequent returns of the hæmorrhage; and upon that ceasing, a serous discharge supervened. Between five and six months ago she began to experience considerable pain and difficulty in passing her stools, attended with tenesmus, and almost constant pain in the gut: her strength was much reduced, with frequent flushings of heat, but her pulse was regular.

"On examination, I found great irregularity and induration in the rectum, about an inch from the anus, which extended some way up the

the gut, when a considerable contraction was discovered, but yet a sufficient passage to admit the tip of the finger being introduced: the contracted part had an irregular and indurated feel. That I might have the patient more immediately under my care, she was admitted an in-patient at the Bath City Infirmary on the 25th of February. The next day a small tent was introduced, and the following pills were prescribed:

℞. Extr. conii, ℨiſs.
Pil. hydrarg. ℨſs. M. f. pil. xxx æquales divid.
quarum capt. ij. mane et vespere.

"A clyster, with gruel and castor-oil, was also directed to be thrown up daily. Her diet—gruel, broth, arrow-root, and light puddings.

"Feb. 27th.—Has had several motions without the injection, and less pain—tent again introduced.

Capt. pil. extr. conii, et pil. hydrarg. j. mane et vespere,
et quoque pil. opiat. gr. j. o. n. h. s.

"28th.—Has a troublesome cough, breathing short, with wheezing. Omittr. pil. hydrarg. &c.

℞. Liquor ammon. acet.
Aq. menth. pip. āā ℥iſs.
..... puræ, ... ℥iij.

Syr. papav. alb, oxym. scillæ, āā ℨij. M. f. mist. Capt. coch, ij. ampl. 4ta. quaque hora. Repr. pil. opiat.—A tent introduced.

"29th.—Breathing rather better, and less pain in the rectum. Repr. mixt. et pil. opiat.—A tent introduced. As the bowels had not been freely open, an injection was directed.

"March 1st.—Had a good night; the bowels have been moved in consequence of the injection, with scarcely any appearance of blood—A tent introduced.

"2d.—Breathing much worse, and cough more troublesome—pulse quick: has had two or three small loose motions without any blood. Repr. med. et enema laxativ.

"3d.—Her breathing better, and cough not so troublesome: had three motions from the injection, but no blood.

"4th.—Much the same: has frequent loose stools, (so as to prevent introducing the tent) but unattended with pain.

"5th.—She has still a frequent discharge of loose stools. Injecr. enema opiat.

"6th.—Breathing much worse, with increase of wheezing, and the cough more troublesome; skin hot and pulse quicker; tongue white, and complains of thirst. A very large quantity of consistent fæces has passed. Applicr. emp. canth. sterno. Repr. mixtur.; add spt. æther vitriol comp. ℨij.

"7th.—Breathing somewhat relieved, but the feverish symptoms continue. Has had two small loose motions. Repr. mist. et capt. haust. anodyn. h. s.

"8th.—Both breathing and cough better; pulse not so quick, and tongue cleaner. Has had three small motions with a little blood. Repr. enema opiat. On introducing a tent, I perceived a fœtid discharge from the rectum, which I had not before noticed. Repr. med.

"11th.—

"11th.—Has very little uneasiness in the rectum, but general pains over the abdomen. Cough and breathing still troublesome, though in a slighter degree. Not so much heat on the skin, nor quickness of pulse. Repr. med. et enema opiat.

"12th.—Less pain over the abdomen. Although there is less heat on the skin, she complains more of thirst. Has had some small loose motions. Repr. enema laxativ.

"13th.—Has very little pain in the abdomen. The clyster occasioned several loose motions, which very much relieved her—a tent introduced.

"14th.—Breathing more affected; has had several loose motions. Applicr. emp. canth. sterno, et repr. med.

"15th.—Breathing somewhat relieved, but the cough still troublesome: has had two loose motions, besides what is found to pass away involuntarily on returns of cough—a tent introduced.

"16th.—Had a restless night, from the difficulty of breathing and cough: passed several sanious colored loose motions. Repr. med.

"17th.—Breathing and cough much the same, but now attended with an expectoration free and copious—has had two small loose motions of the same appearance as last. Repr. med. et enema laxativ.

"18th.—Breathing much the same; a little bloody mucus is brought up with the cough: has had more uneasiness in the bowels. Two injections have been given without producing any effect—the injection was ordered to be repeated with the addition of a little murias sodæ.

"19th.—Had no evacuation until she took some castor-oil this morning, which procured several motions: cough and breathing much the same. Repr. med.

"20th.—Had a better night: breathing not so difficult; skin cool; pulse regular; tongue clean. Passed three stools without any pain. Repr. med.

"22d.—Her breathing much better, and cough not so urgent: had a very good night; bowels open—a small tent introduced.

"24th.—Continues better: bowels still in an open state, and the evacuations of a more natural consistence—a tent introduced.

"26th.—Bowels having been confined yesterday has taken castor-oil, which procured three motions, one of them very copious—a tent introduced.

"27th.—Her breathing and cough better: bowels open—felt a soreness in the rectum after the tent yesterday.

"28th.—Feels better: has had two motions without pain—a tent introduced.

"30th.—Having had no evacuation yesterday took castor-oil, which operated two or three times.

"31st.—Complains of sickness, and the having brought up bile: had a motion this morning, followed by a little blood. The fœtid discharge from the rectum has ceased.

Capt. mist. salin. card. ℥i. 4tis. horis. Capt. haust. Anodyn. h. s.—a tent introduced.

"April

" April 3d.—Sickness better: complains of pain over the abdomen. Took castor-oil yesterday, which procured several motions—a tent introduced.

" 5th.—Less pain in the abdomen: bowels open, and the fæces discharged without pain—a tent introduced.

" 9th.—A tent introduced.

" 11th.—Complains of having had a considerable soreness in the rectum since the last tent was introduced. Although, on examination with the finger, the contraction does not appear increased, yet there is a greater difficulty in passing the tent from the extreme irregularity on the internal surface of the gut—the tent was omitted. The bowels were kept open with castor-oil, and the evacuations continued to be discharged without pain or appearance of any blood. Her general health appeared also to be improving, and she was able to sit up a few hours daily, which she had not been able to do for a long time: her appetite was so much better as to render her very desirous of having a little animal food, which was complied with.

" On the 23d a tent was introduced, but could not pass it until I had previously ascertained the direction of the contracted part by introducing the finger, the irregularity of the surface continuing the same.

" 25th.—The tent occasioned considerable pain in the rectum, and a little blood followed its removal. She took castor-oil this morning, not having had a motion since the last tent was introduced.

" 26th.—Had several motions yesterday, and her bowels very open to-day: does not complain of any particular pain.

" 28th.—A small tent again introduced—the last time.

" 30th.—Complains of having had much soreness in the rectum since the last tent was introduced, and has had no motion. Rep. enema laxativ.

" May 1st.—Passed several motions. Had appeared to be rather weaker, her appetite having failed for the last day or two; but no material alteration was observable until the 5th, when, on entering the ward in the morning, I was surprised to find so great a change in her countenance; her breathing short; pulse extremely feeble; with every other appearance of a speedy dissolution. She died the same afternoon. The nurse informed me she had become suddenly worse in the night.

" *Appearances on dissection.*—On dividing the parietes of the abdomen, there were evident marks of peritoneal inflammation, and the intestines also exhibited a similar appearance, but more particularly the ilium, and its folds were glued together in several places, the consequence of inflammatory exudation; and on its surface there were different patches of coagulable lymph: there was also some purulent matter in the pelvis. On separating the rectum from the sacrum, its posterior part gave way, appearing that only the peritoneal coat at this part of the intestine had remained; the other coats having been destroyed by ulceration. The internal surface of the gut was extremely irregular, and its inner membrane entirely destroyed by ulceration; which process had extended somewhat less than an inch from the anus, as far as the contracted portion of the rectum. The

 muscular

muscular coat was very much thickened and indurated, exhibiting the usual cancerous appearance: and in other places (besides the posterior part already noticed) it appeared to be entirely destroyed, as well as the inner coat, by the ulcerative process. At the termination of the ulceration there was a considerable contraction of the gut, from the diseased state of the muscular coat having formed a complete thick cartilaginous ring; and a little below it the jagged edges of the inner coat projected; its lower portion, as before mentioned, being entirely destroyed by ulceration. Above the cartilaginous ring the intestine was somewhat dilated, its inner membrane having an inflamed appearance, which had extended about two inches up the gut. The muscular and peritoneal coats, at the back part of the superior portion of the rectum, were thickened and indurated, extending in a line along the sacrum for nearly three inches above the contraction; the thickening gradually lessening as it extended upwards. A great quantity of solid fæces was collected above the contracted part, and properly tinged with bile.

"About the middle of the convex surface of the liver there was a very large tubercle, with several lesser ones dispersed throughout its substance.

"The fundus uteri was red, and the fimbriated extremities of the fallopian tubes were in a state of ulceration; no doubt from having been exposed to the purulent matter which was collected in the pelvis.

"The lungs had a diseased appearance, and with some difficulty separated from the back part of the thorax."

We learn from the introductory part, that the alvine discharges were always of a light clay color in this woman, except when calomel was taken; but in the detail of the case, it does not appear that calomel was given under the direction of Mr. White, or the fæces noticed as regards color. Dissection certainly showed a tuberculated state of liver; but neither the history or dissection remove the hypothetical character of the opinion. The investigation will probably be further pursued, and the sympathetic connection between strictured rectum and tuberculated liver, we hope, will be established on more extended evidence.

Observations on Peritonitis, and on some other internal Inflammatory Affections. By THOMAS SUTTON, M.D. of the Royal College of Physicians, late Physician to the Forces, and consulting Physician to the Kent Dispensary.

(*Continued from p. 336.*)

THE merit of the author in this part of his work, which consists in a relation of cases, is principally founded on his directing a successful mode of practice in complaints which often prove tedious, difficult of cure, and even fatal. A case

of

or two will show the value of the author's observations and practice.

"A gentleman consulted me on the subject of some obscure pains he experienced on the right side, midway between the ribs and fore part of the ilium, which were not then increased upon pressure. He said he had, for some time, labored under a disease which his medical advisers in town had not formed any precise opinion about; but he had been cupped, used the warm bath, and had taken opening medicines, without perceiving any other than temporary advantage. He had since applied to another source, and been dissatisfied with the result. The patient complained that he felt uneasiness with any thing of a shaking motion; but as he came to me in his carriage, and did not immediately meet with me at home, he had driven about under the idea of falling in with me. I found his countenance good, the pulse natural; but he expressed himself to be extremely uncomfortable and low, with depression of spirits, want of appetite, and when he did eat he became worse after it. I directed some opening medicines, and a soap plaster to be applied to the part affected; but, before the medicines could be got home, I was sent for, and found the patient in considerable pain, augmented on pressure. He was directed leeches and fomentations to the part, and afterwards a blister, and the bowels to be freely acted upon. The pain wore off to a certain extent; but in two days I was sent for in some haste, and found the patient in great pain, in a profuse clammy sweat, with a pulse of 120. Under these circumstances, I judged it proper to recommend blood to be drawn to sixteen ounces, which quantity was exceeded, and which afforded much relief, though the blood was not sizy, but tough. From this time the disease gradually got better, but returned twice, with the interval of a fortnight between the attacks. The third being violent, I considered it to be adviseable again to recommend venesection to some amount, which occasioned relief; but the second attack was comparatively mild, and the loss of blood was trusted to leeches. The other part of the treatment consisted in the use of warm fomentations, blisters, and purgative and aperient medicines, chiefly with sulphate of magnesia, and infusion of senna. The patient has had no further severe returns of this disease, though for some time he was frequently unwell, and obliged to use much caution in his mode of living and exercises. The disease, as appears, came on in a lurking obscure manner, was some considerable time before it arrived at a violent paroxysm, and was not subdued for upwards of six weeks, though much abated in the intervals of the attacks.

"The expressed seat of the disease was at a considerable distance from the kidnies, and in a line drawn from the anterior spinous process of the ilium, parallel to the linea alba. The seat of the disease is more particularly stated, because, previous to these violent attacks, the patient had perceived small quantities of blood to be discharged once or twice with the urine, though nothing of the kind had occurred during the period just detailed, when the secretion was copious, of proper color, and varying as it is known to do in attacks of fever,

and was every day inspected while any urgent symptoms remained. Afterwards the appearance of blood in the urine was again occasionally observed, though in inconsiderable quantities. This discharge was probably excited by the same cause as a similar one which was noticed in the following case, though originating from different organs."

"Mr. Hurt, apothecary and chemist, residing on Deptford Bridge, about a month previous to the attack, which will be more particularly adverted to, consulted me on the subject of a disease which had been very painful to him for some time, and of which this was the second attack, after a short interval. He described it to be about an inch and an half round the navel. He had been advised to apply leeches, to use fomentations, and to blister the part; from which remedies he thought himself somewhat relieved. Under the circumstances I found him in, I recommended him to keep his bowels well open, and if there should be any material return of the disease, to have blood drawn from the arm to the extent of from sixteen to twenty ounces, which the present state of the habit appeared to me to be capable of allowing. In about a month from this time I was again desired to see him. He stated, that he had never been well from the complaint he consulted me about, but that now he was in great torture. He had considerable febrile heat, coated tongue, and a pulse one hundred and ten and upwards, with nausea and vomiting. The local affection was in extent nearly circular, with a diameter of about three inches, making the navel its centre. The sensation of the patient was that of a burning heat, with shooting pains, and a feeling of great weight in the part. The patient had had recourse to leeches, blisters, and fomentations, as before, but with no effect. I recommended blood to be drawn to the extent of sixteen ounces, and the bowels to be kept open. He was afterwards bled five times, to twelve, ten, and eight ounces, as the symptoms appeared to require, without any satisfactory progress being made. In addition, the part was fomented with the decoction of poppies. This was the treatment pursued during the first week of my attendance, and to about the tenth day of the violent return of the disease. It was next determined to try the effects of more active purgatives than had hitherto been done, with the addition of small doses of opium; the latter for the purpose of relieving pain, and repressing the tendency to vomit. By this plan, the bowels appeared to be completely evacuated of their contents, but without any material amendment ensuing. Recourse was then had to considerable doses of opium to relieve the disease, which had certainly the effect of mitigating pain, while the patient was under its powerful influence; but it did not appear to diminish the disease otherwise. Local bleeding was then employed again, and a blister applied, but these produced no perceptible advantage. The disease had now continued about three weeks. The local affection was very little abated; the pulse was somewhat diminished in frequency; and a fulness and hardness painful to the touch in the part affected, within the limits above described, had become much more evident, and particularly attracted the attention of the patient. Under these circumstances,

stances, I suggested the use of a common emollient poultice, but found it increased the local affection considerably. On this report, I determined to recommend the patient to employ a lotion composed of equal parts of aq. ammoniæ acet. and water, with half an ounce of rectified spirits to eight ounces. In two days afterwards, I called, and was gratified to find the patient much amended. He stated, that, from the very first application of the lotion, he had experienced relief, and that his pain returned only occasionally, wheu the use of the lotion repressed it. From this time the patient might be considered to be convalescent. The only things afterwards advised, were to keep the bowels open, to be moderate in the use of food, to avoid fermented liquors, and to use the lotion as frequently as might be found necessary. Since which time, (about a year,) I have uniformly heard a good account of Mr. Hurt's health."

Several other cases are related, attesting the good effects of cold applications in peritonitis, and in some affections of the chest. One case even proves the advantage of cold sea-bathing in consumption. We were much gratified with this part of the work, and congratulate the author on his resolution in combating both vulgar and professional prejudice, as well as upon the success which attended his practice. What will the hot-house practitioners think of applying a cold lotion to the chest in consumptive cases, which Dr. Sutton very coolly proposes? He has favored us with some interesting particulars of the practice of Dr. Stewart, a clergyman, who has obtained such high reputation in Scotland for curing consumption, as to attract the notice of a noble family in London, who consulted him in consequence of his decided success. Will it be credited that part of his plan consists in applying cold vinegar and water to the chest daily, which he also recommends to be well rubbed?

The injurious consequences of wearing too much flannel are illustrated by a case in which the patient consulted Dr. Sutton on account of a cough which had troubled him for some time, and by which he was becoming weak and emaciated.

"After an attendance of about ten days, the symptoms had grown worse. The patient was now so weak that he could with difficulty leave his bed: he had a very harassing cough, with an expectoration of an uncertain character, hectic fever, and profuse perspirations, with a pulse of one hundred and twenty. About this time, in one of my visits, the conversation turned on the state of the patient's health some years before, which was represented to be delicate, and that he was subject to rheumatic affections in various parts of his body, on exposure to cold. On this account, he wore great quantities of flannel, which led me to inquire into the state of his present clothing. I found he was completely invested in flannels of various and the warmest kinds, and that he constantly wore several such garments.

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As it appeared to me that this sort of clothing must be very prejudicial to him in his present state, and particularly by encouraging profuse perspirations, I recommended all but one of these garments to be dismissed, and, as soon as possible, to substitute for the remaining one a coarse calico waistcoat. After this change had been adopted, the patient very rapidly and completely recovered, which was plainly due to the diminished heat on the surface."

The remaining portion of the volume is devoted to the consideration of gout; and if personal experience is a recommendation, the doctor's authority is good, for his own sufferings in that complaint induced him to pursue bold and active treatment. In fact his own cure forms the chief part of the essay. He commences with some general observations upon the disease, which tend to dispose the reader to admit that something more may be done both in alleviating the pain, and even curing the disease, than merely leaving the patient to nature and despair. The remarks on this subject are supported by various authorities, and we fully concur with him in opinion that much may be done towards mitigating the violence, and removing the complaint, at least for a time.

The remedies from which he experienced the most decided benefit were strong purgatives. He afterwards, however, was so situated that he could not conveniently take them, and had recourse to a large dose of laudanum, which completely succeeded in subduing his gouty paroxysm.

From some experience, and also having seen the practice adopted by others, we have long been of opinion that opium taken in doses sufficiently large to ease the pain and induce sleep, followed up by suitable and adequate purgatives, are strongly to be relied upon in all cases of gout where the practitioner sees no indication to forbid their use. We are, therefore, pleased to find this practice, which at the same time we must remark is no new practice, corroborated by the statement of Dr. Sutton.

A Practical Synopsis of Cutaneous Diseases according to the Arrangement of Dr. Willan, exhibiting a concise View of the Diagnostic Symptoms, and the Method of Treatment. By Thomas Bateman, M.D. F.L.S. Physician to the Public Dispensary and to the Fever Institution.—8vo. pp. 342. Longman and Co. 1813.

Considering the advances made by the ancients towards a knowledge of cutaneous affections, it seems remarkable that so little should have been accomplished by the moderns in this interesting department of medical science. To the ancients

cients we are indebted for most of the names which at present distinguish cutaneous diseases, and to such authority, till very lately, we were compelled to resort for their description and treatment. Dr. Willan, the latter end of last century, presented us with the first number of a voluminous work upon this subject, of which he did not live to complete more than half the intended series. But that half was the most important; the general outline was sketched, though the master-hand was only put to the first part. In this publication he displayed an acquaintance with the writings of the ancients highly creditable to his erudition, and evinced an acuteness of perception, an accuracy of distinguishing, combined with scientific arrangement, worthy of the enlightened physician. From the rude mass of heterogeneous matter before him, he organized a rational system, and enriched it with new observations of his own. The difficulty of the subject is demonstrated by the fact, that eager as well-informed medical men usually are in the pursuit of objects which may signalize them, the subject of cutaneous diseases had received very little elucidation since the obscure writings of the Arabians, until Dr. Willan happily selected it as deserving his attention, and his success has shewn that his talents were equal to the laborious investigation. It is true that most practitioners were in some degree acquainted with the appearances, and knew something of the mode of treating some of the more frequent and prominent cutaneous diseases; but the most candid will be ready to allow that they often guessed at what is now reduced to certainty, and often acted empirically when they might have proceeded upon indubitable and scientific principles. If we were for a moment to admit, which indeed we cannot in truth, that the practice is not materially improved, there can be no doubt whatever that the knowledge of the nature and progress of cutaneous complaints is much advanced by Dr. Willan's valuable work. We can now at least promptly decide what the event of many cases will be, whether they are curable or not; and it is of not the least importance too, to give them a name, about which, what with the plates, the definitions, and the descriptions, we need not long hesitate. But Dr. Willan having projected eight orders of cutaneous diseases, only finished the description of four; and here would have been a lamentable deficiency indeed, had not an able successor in this line of practice immediately presented himself to occupy the chasm which would otherwise have occurred. Dr. Bateman, who, besides his well known intimate acquaintance with medicine, appears to have devoted a considerable degree of attention to cutaneous disorganization in particular, possessed the great advantage of

enjoying

enjoying the confidence of Dr. Willan, witnessing his practice, and discussing his peculiar opinions and arrangement, has, with a zeal and promptitude which must ensure success, got out the volume before us.

In the preface he cautions us not to consider it as the completion of Dr. Willan's treatise. "Its sole purpose," he observes, "is to present an abstract of the classification proposed by that respected author, together with a concise view of all the genera and species which he intended that it should comprehend. The materials for the description of the first four orders have been obtained principally from Dr. Willan's publication, of which the first part of this synopsis may be regarded as an abridgment. Some additional facts, however, have been supplied from subsequent observation. The remainder of the matter has been derived partly from personal experience and research; but principally from a constant intercourse with Dr. Willan upon the subject of these diseases, during a period of ten years, while his colleague at the Public Dispensary, and from his own communications in his last illness."

Before quoting the definitions, it is right to state that a colored engraving, which for the subject is certainly beautiful, exhibits the eight forms of cutaneous eruptions, and also illustrates some of the genera and species in a very happy manner.

DEFINITIONS.

"1. Papula (*Pimple*), a very small and acuminated elevation of the cuticle, with an inflamed base, very seldom containing a fluid, or suppurating, and commonly terminating in scurf.*

"2. Squama (*Scale*), a lamina of morbid cuticle, hard, thickened, whitish, and opaque. Scales, when they increase into irregular layers, are denominated crusts.

"3. Exanthema (*Rash*), superficial red patches, variously figured, and diffused irregularly over the body, leaving interstices of a natural color, and terminating in cuticular exfoliations.

"4. Bulla (*Bleb*), a large portion of the cuticle detached from the skin by the interposition of a transparent watery fluid.

"* The term, Papula, has been used in various acceptations by the older writers, but the nosologists have nearly agreed in restricting it to the sense here adopted. Sauvages defines it, 'Phyma parvulum, desquamari solitum.' (Nosol. Meth. class i. Synops. ord. ii. 6. See also Linnæi Gen. Morbor. class xi. ord. 4.)—In this sense also Celsus seems to have understood the term, although he uses it generally; for when he calls it a disease in which 'the skin is made *rough* and *red* by very minute pustules,' he means obviously dry papulæ; as by the word pustula he understands every *elevation* of the skin, including even wheals. (De Med. lib. v. cap. 28, § 15 and 18.)"

"5. Pus-

"5. Pustula (*Pustule*), an elevation of the cuticle, with an inflamed base, containing *pus.*

"Four varieties of pustules are denominated in this arrangement as follows:

"*a. Phlyzacium*, a pustule commonly of a large size, raised on a hard circular base, of a vivid red color, and succeeded by a thick, hard, dark-colored scab.*

"*b. Psydracium*, a small pustule, often irregularly circumscribed, producing but a slight elevation of the cuticle, and terminating in a laminated scab.† Many of the psydracia usually appear together, and become confluent; and, after the discharge of pus, they pour out a thin watery humor, which frequently forms an irregular incrustation.

"*c. Achor*, and

"*d. Favus.* These two pustules are considered by the majority of writers from the Greeks downwards, as varieties of the same genus, differing chiefly in magnitude.‡ The *achor* may be defined a small acuminated pustule, containing a straw-colored matter, which has the appearance and nearly the consistence of strained honey, and succeeded by a thin brown or yellowish scab. The *favus*, or κηριον, is larger than the *achor*, flatter, and not acuminated, and contains a more viscid matter; its base, which is often irregular, is slightly inflamed; and it is succeeded by a yellow, semi-transparent, and sometimes cellular scab, like a honey-comb, whence it has obtained its name.

"6. Vesicula (*Vesicle*), a small orbicular elevation of the cuticle, containing lymph, which is sometimes clear and colorless, but often opaque, and whitish or pearl-colored. It is succeeded either by scurf, or by a laminated scab.

"* The derivation of this term, 'απο τȣ φλυω, φλυζω, sive φλυσσω, quod *fervere* significat, et *ebullire*,' (Gorræi Def. Med.) would render it sufficiently appropriate to elevated and inflamed pustules, if we had not possessed also the interpretation left by Celsus: 'φλυζακιον autem paulo durior pustula est, subalbida, acuta; ex qua quod exprimitur, humidum est. Ex pustulis vero nonnunquam etiam ulcuscula fiunt, aut aridiora, aut humidiora: et modo tantum cum prurigine, modo etiam cum inflammatione aut dolore; exitque aut pus, aut sanies, aut utrumque. Maximeque id evenit in ætate puerili; raro in medio corpore; sæpe in eminentibus partibus.' (De Medicina, lib. v. cap. 28, § 15.)"

"† As the *Phlyzacia* were denominated from the heat of the eruption, so the *Psydracia* received their appellation from the opposite quality, 'quasi ψυχρα ὑδρακια, id est, *frigidæ* seu frigefactæ *guttulæ*,' says Gorræus.—The psydracia are enumerated among the eruptions peculiar to the head by Alexander and Paul, and some other Greek writers; but Galen and others mention them as common to other parts of the body. (See Alex. Trall. Op. lib. i. cap. 5. Paul. Ægin. lib. iii. cap. 1. Actuarius, lib. vi. cap. 2.)"

"‡ See Aëtius, tetrab. ii. serm. ii. cap. 68.—Alex. Trall. lib. i. cap. 8 and 9.—Paul. Ægin. de Re Med. lib. iii. cap. 3.—Oribas de Loc. Affect. lib. iv. cap. 12."

"7. Tu-

"7. TUBERCULUM *(Tubercle)*, a small, hard, superficial tumor, circumscribed, and permanent, or suppurating partially.

"8. MACULA *(Spot)*, a permanent discoloration of some portion of the skin, often with a change of its texture.

"The following terms are used in their ordinary acceptation, viz.

"9. *Wheal*, a rounded or longitudinal elevation of the cuticle, with a white summit, but not permanent, nor containing a fluid, nor tending to suppuration.

"10. FURFUR *(Scurf)*, small exfoliations of the cuticle, which occur after slight inflammatioa of the skin, a new cuticle being formed underneath during the exfoliation.

"11. SCAB, a hard substance covering superficial ulcerations, and formed by a concretion of the fluid discharged from them.

"12. STIGMA, a minute red speck in the skin, without any elevation of the cuticle. When stigmata coalesce, and assume a dark-red or livid color, they are termed *Petechiæ*."

As many of our readers may not be acquainted with Dr. Willan's arrangement, we shall lay it before them, and then subjoin a specimen of Dr. Bateman's valuable performance, which ought to be in every one's hands.

"The diseases of the skin were arranged by Dr. Willan in eight orders, according to their external forms above defined, as in the following table.

Order I.—PAPULÆ.

Strophulus.	Prurigo.
Lichen.	

II.—SQUAMÆ.

Lepra.	Pityriasis.
Psoriasis.	Ichthyosis

III.—EXANTHEMATA.

Rubeola.	Roseola.
Scarlatina.	Purpura.
Urticaria.	Erythema.

IV.—BULLÆ.

Erysipelas.	Pompholyx.
Pemphigus.	

V.—PUSTULÆ.

Impetigo.	Variola.
Porrigo.	Scabies.
Ecthyma.	

VI.—VESICULÆ.

Varicella.	Miliaria.
Vaccinia.	Eczema.
Herpes.	Aphtha.
Rupia.	

Order

VII.—TUBERCULA.

Phyma.	Sycosis.
Verruca.	Lupus.
Molluscum.	Elephantiasis.
Vitiligo.	Frambœsia.
Acne.	

VIII.—MACULÆ.

Ephelis.	Nævus, Spilus, &c.

Where every part of a work is excellent, we are spared the necessity of criticism, and can have no hesitation about selecting specimens, for there is no occasion for choice. We cannot abridge, for Dr. Bateman has condensed his information as much as possible: it only remains for us, then, to recommend the work in the most unqualified manner. At the same time we must not be supposed to insinuate that the work is perfect, for we have no doubt that the learned author will very shortly acquire still more extensive practical knowledge upon the subject; but we consider it the only book extant that contains a comprehensive yet explicit account and scientific arrangement of the diseases of the skin; keeping always in mind that Dr. Willan's great work was only half completed.

We think Dr. Bateman might have said more upon Lupus, for in proportion as the disease is rare, and of difficult cure, he should have exerted himself to collect more particulars than he has accomplished. His alleged reason appears to us merely a *ruse* to avoid delay in getting out his book: he hastily observes, "Of this disease (Lupus) I shall not treat at any length, for I can mention no medicine which has been of any essential service in the cure of it; and it requires the constant assistance of the surgeon, in consequence of the spreading ulcerations in which the original tubercles terminate." We have seen a very severe case of this formidable disease, which had extended over nearly half the body, and was gradually advancing, completely checked, and finally destroyed, by the application of caustic. We have no doubt Dr. Bateman will have more to say upon this disease on a future occasion, and that he will not slur over the diseases resembling syphilitic appearances, although not always connected with venereal poison, as his laudable haste in furnishing the public with the present volume has probably induced him to do on this occasion. *Non omnia possumus omnes*, and Dr. B. has atchieved more in a given time than his warmest friends could have expected. He will therefore, after the manner in which we have stated the impression his book made upon us, no doubt pardon these hints.

Some readers will not purchase without a sample, and, as we before observed, the work will not admit of any curtailment, we shall copy the author's account of Lepra, the first genus of the second order SQUAMÆ.

"The term Lepra is here appropriated solely to the leprosy of the Greeks, as described by the more accurate of those writers. It is characterised by 'scaly patches, of different sizes, but having always nearly a circular form.*'

"1. Lepra *vulgaris*, the ordinary species of the disease in this country, commences with small, round, reddish, and shining elevations of the skin, at first smooth, but within a day or two exhibiting thin white scales on their tops. These gradually, sometimes rapidly, dilate to the size of half-a-crown, still retaining their oval or circular form, and are covered with shining scales, and encircled by a dry, red, and slightly elevated border. In some cases these scales accumulate so as to form thick prominent crusts. If the scales or crusts are removed, the skin appears red and shining, being very smooth, and free from the cuticular lines in the beginning, but marked, in the advanced stages, with long deep lines and reticulations, not always coinciding with those of the adjoining surface.

"The lepra most commonly commences on the extremities, where the bones lie nearest to the surface, especially below the elbow and the knee, and usually on both arms, or both legs, at the same time. From these points it gradually extends, by the formation of new and distinct patches, along the arms or thighs, to the breast and shoulders,

"* The confusion which has every where prevailed in the use of the terms *lepra* and *leprosy*, seems to have originated principally with the translators of the Arabian writers after the revival of learning. The Greeks agreed in appropriating the appellation of λεπρα to a *scaly* eruption (as its etymology dictated): most of them deemed it the highest degree of *scaliness*, exceeding in this respect the Lichenes, Psora, and Alphos; and those who were most minute in their description, stated that 'it affects the skin deeply, in *circular* patches, at the same time throwing off scales like those of large fishes.' (See Paul. Ægin. de Re Med. lib. iv. cap. 2; and Actuarius, de Meth. Med. lib. ii. cap. 11: also Aëtius, tetrab. iv. serm. i. cap. 134; and Galen. Isagoge.) This was sufficiently clear; but those who translated the works of the Arabians into Latin, fell into the extraordinary mistake of applying the Greek term to a *tubercular* disease, which had been actually described by the Greeks under the appellation of Elephantiasis; and they applied the barbarous term *Morphœa*, together with Scabies and Impetigo, to the scaly diseases of the Greeks above enumerated. Whence their followers, who detected the error, spoke of the Lepra Arabum as well as the Lepra Græcorum; while the less accurate confounded every foul cutaneous disease under the term leprosy. The Arabians themselves do not employ the word Lepra, but have described these different diseases under appropriate appellations."

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and to the loins and sides of the abdomen. In several cases I have observed the eruption most copious and most permanent round the whole lower belly. The hands also become affected, and in many cases the hairy scalp; but the face is seldom the seat of large patches, although some scaliness occasionally appears about the outer angles of the eyes, and on the forehead and temples, extending from the roots of the hair. In the more severe cases, the nails of the fingers and toes are often much thickened, and become opaque and of a dirty yellowish hue, and are incurvated at the extremities: their surface is also irregular, from deep longitudinal furrows, or elevated ridges.

"When the eruption of lepra is moderate in degree and extent, it is not attended with any uneasy sensations, except a slight degree of itching when the patient is heated by exercise, or becomes warm in bed; and a little occasional tingling in certain states of the atmosphere.* When it is generally diffused, however, and there is a considerable degree of inflammation in the skin, it is accompanied with extreme soreness, pain, and stiffness, which I have sometimes seen so great as to render the motions of the joints impracticable, and to confine the patient to bed. Yet even under these circumstances there is no constitutional disturbance; and if no medicine be employed, the disease of the skin may continue for months, or even years, without any material derangement of the system.

"It is not easy to point out the causes of this disease, which appear, indeed, to be very various; for it is one of the most common affections of the skin, at least in this metropolis, and occurs at all periods, and under every circumstance of life.† It is certainly not communicable by contagion, nor does it appear to originate from confinement to certain kinds of diet, such as fish, dried or salted meats, &c. since it is not endemic in districts where these are habitually used, and occurs frequently where they are almost unknown. But, like some other cutaneous affections of a more transient character, it is certainly produced occasionally by the influence of particular articles of food and drink, which operate through the idiosyncrasy of individuals. I have met with one gentleman in whom spices or alcohol speedily produce it. The original attack in him occurred after eating

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"* Hippocrates remarks that some *Lepræ* itch before rain: lib. Περι Χυμων."

"† It is difficult, therefore, to account for the opinion expressed by the late Dr. Heberden, respecting the extreme rarity of lepra in this country. 'De vero scorbuto et lepra, nihil habeo quod dicam, cum alter rarissimus est in urbibus, altera in Anglia pene ignota; unde factum est ut hos morbos nunquam curaverim.' (Comment. cap. 23.) And still more difficult to explain the statement of Dr. Cullen, whose definition of lepra will include both the dry and humid tetters (Psoriasis and Impetigo) with the proper scaly lepra; but who nevertheless affirms that he had never seen the disease. Nosol. Meth. class iii, gen. 88, *note.*"

some hot soup, containing spice, the first spoonful of which excited a violent tingling over the whole head; which was followed by the leprous eruption, which soon extended to the limbs. In another case, in a young gentleman of nineteen, the disease commenced after taking copious draughts of cream; and vinegar, oatmeal, and other species of food, to which it has been ascribed, have probably given rise to it occasionally: but these are all anomalies, and are only referable to peculiar idiosyncracy.* In some cases it has commenced after violent and continued exercise, by which the body had been much heated and fatigued.

"Dr. Willan has imputed the origin of lepra to cold and moisture, and to certain dry sordes on the skin. It has seldom occurred to me, however, to witness the disease in bakers, laboratory men, and others who work among dry powdery substances; while I have observed a considerable number of cases in young ladies, and in persons of both sexes in respectable ranks of life, by whom every attention to cleanliness was scrupulously paid. Where cold and moisture have excited the eruption of lepra, the predisposition to it must have been peculiarly great. On the whole, the causes of this disease are involved in much obscurity. There is obviously an hereditary predisposition to it in some individuals.

"2, Lepra *alphoides*†. This is a less severe form of the disease than

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"* Some poisonous substances taken into the stomach have produced an eruption of lepra. The poison of copper is stated to have speedily excited it in several persons at the same time, in one of whom it continued for a month, but disappeared in the others in about ten days. See Med. Facts and Obs. vol. iii. p. 61."

"† The Greeks have described the *Alphos* as a milder disease, being more superficial, and less rough, than the lepra: (see Galen, de Sympt. Caus. lib. iii.—Aët. tetrab. iv. serm. i. cap. 134.) and the description of it given by Celsus accords with the appearances of the L. alphoides above stated. 'Αλφος vocatur, ubi color albus est, fere subasper, et non continuus, ut quædam quasi guttæ dispersæ videantur. Interdum etiam latius, et cum quibusdam intermissionibus, serpit.' (de Medicina, lib. v. cap. 28.) Celsus no where employs the term lepra.

"This scaly *Alphos*, which was deemed by Hippocrates a blemish, rather than a disease (Περι Παθων, sect. 15), was distinguished from another *white* affection of the skin, the *Leuce*, which was not scaly, but consisted of smooth shining patches, on which the hairs turned white and silky, and the skin itself, and even the muscular flesh underneath, lost its sensibility. The *Leuce* was a disease of an incurable nature. (Hipp. Προῤῥητικ. lib. ii.) Celsus, although pointing out this distinction, includes the Leuce and the Alphos under the same generic title, Vitiligo. (loc. cit.)

"It may be remarked that the Arabians distinguished these two affections by different generic appellations, calling the Alphos *Albohak*, and

than the preceding. It differs chiefly in the small size of the patches, which seldom extend beyond the diameter of a few lines, or become confluent,—in the minuteness and greater whiteness of the scales,—and in its limitation to the extremities. This variety of lepra is most common in children. It is tedious and difficult of cure, like the former, and requires similar treatment.

"It would be superfluous to enumerate the catalogue of useless medicines which have been recommended from ancient times for the cure of lepra: I shall, therefore, confine my attention to those, of the beneficial agency of which I can speak from experience. It is necessary to premise, however, that there is no one remedy, nor any invariable plan of treatment, which will succeed in lepra, under all the circumstances of its appearance in different instances; and that great errors are committed by prescribing for the name of the disease. The circumstances to which I allude more particularly, are the different degrees of cutaneous excitement, or inflammatory action, which accompany the disease in different habits; and which, if carefully attended to, afford an important guide to the most successful application of remedies.

"In the less irritable conditions of the leprous eruption, such as the L. alphoides usually exhibits, as well as a few cases of the L. vulgaris, a gently stimulant mode of treatment, at least externally, is requisite; though in all cases of lepra the diet should be light and moderate, and heating liquors should be avoided, especially malt liquors and spirits; for every indulgence in these points will be felt in the aggravation of the symptoms. A frequent use of the warm bath, with which a moderate degree of friction may be combined, contributes to remove the scales, and to soften the skin; or, if the eruption be confined to the extremities, local ablution and friction may be sufficient. These cases are benefited by the use of the sulphur waters of Harrowgate, Leamington, Crofton, and other well-known springs, both internally and externally, and by the warm sea-water bath. In fact, these gently-stimulant ablutions are often sufficient, if persevered in during several weeks, to remove the modifications of lepra of which I am now speaking.

"But if the scales adhere tenaciously, or are accumulated into thick crusts (see Def. 2), then some more active lotion must be con-

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and the Leuce *Albaras*, with the epithet *white*. Their translators have called the former *Morphœa*, and included the Leuce and Elephantiasis under the appellation of *Lepra*. By retaining these distinctions in recollection, the accounts of the older writers may be read, while the confusion arising from their misapplication of names may be avoided.

"It appears probable, that the *Leuce* was the leprosy of the Jews, described in Leviticus, chap. xiii. See Greg. Horstii Obs. Med. lib. vii. p. 330.—Leon. Fuchsii Paradox. lib. ii. cap. 16.—Th. Campanellæ Ord. Medic. lib. vi. cap. 23.—Hensler, vom Abendländischen Aussatz, p. 341."

joined

joined with the warm ablution, or with the application of steam, in order to clear the surface. Lotions of diluted alcohol, of sulphurated potass, or the decoction of dulcamara, will aid the exfoliation; and the thick crusts may be softened and loosened by lotions containing a portion of the liquor potassæ, or of the muriatic acid. When these are removed, the cuticle may be restored gradually to its healthy condition, by the unguentum picis, or the unguentum hydrargyri nitratis diluted with saturnine cerate, or simple ointment; or lotions containing a small proportion of the oxymuriate of mercury may be substituted. The ointments should be applied at night, and washed off in the morning with warm water, or a slight saponaceous lotion.

"The same cases will be accelerated in their progress towards a cure, by the use of those internal remedies which tend to support the strength, and to stimulate the cutaneous vessels. For this purpose the arsenical solution,* recommended by Dr. Fowler, is often extremely beneficial, in doses of four or five drops, which may be slowly increased to eight, and persevered in for a month or more.† Pitch, administered in the form of pills, is productive of a similar good effect, where the cutaneous circulation is very inert; but both these medicines are liable to aggravate the eruption, where it is connected with much irritability of the skin. The solution of oxymuriate of mercury has appeared to have some efficacy in these inert states; and in thin and delicate girls, of relaxed habit, affected with the lepra alphoides, the vinum ferri, or the tartrite before-mentioned, has been taken with much advantage.‡

"One of the most effectual remedies for lepra, however, under all its varieties, is the decoction of the leaves and twigs of the solanum dulcamara, which was introduced to the notice of British practitioners by Dr. Crichton.§ This medicine is at first administered in doses of

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"* Preparations of this mineral have a direct tendency to stimulate the cutaneous circulation, and to inflame the skin; and are, therefore, altogether inadmissible in the irritative forms of lepra."

"† This active medicine being now not only sanctioned by the profession in general, but by the Pharmacopœia of the College, it will be enough to state, that, in these smaller doses, which experience has proved to be sufficient, it may be taken without any inconvenience. Another preparation, introduced by the late Dr. de Valangin, is kept at Apothecary's Hall, under the name of solutio solventis mineralis, and is equally efficacious in smaller doses."

"‡ If in any case the tinct. lyttæ prove useful in lepra, it would probably be in these more inert instances. But it is to be observed that Dr. Mead, who originally recommended this medicine, was speaking, not of the scaly Lepra, but of the Leuce, or of the Elephantiasis. See his Medicina Sacra, cap. ii."

"§ See his communication to Dr. Willan. (Treatise on Cutan. Diseases, p. 145.) His formula has been adopted by the College in the late edition of the Pharmacopœia."

two

two or three ounces thrice every day, which are gradually augmented, until a pint is at length consumed daily. When there is a degree of torpor in the superficial vessels, the same decoction, made with a larger proportion of the shrub, is advantageously employed as a lotion; but if there is much inflammatory disposition, this and every other external stimulus must be prohibited.

"Where this irritable state of the disease exists, indeed, (and it is the most frequent,) nothing more stimulating than tepid water, or thin gruel, can be used for the purposes of ablution; and the arseniates, pitch, &c. above mentioned, must be excluded. The disease, under this condition, will be certainly aggravated by sea-bathing, by the external use of the strong sulphureous waters, or of any irritant, as I have frequently observed; but it will be alleviated by the internal employment of sulphur, with soda or nitre, or the hydrarg. sulphuratus niger with an antimonial, especially when conjoined with the decoction of dulcamara. The caustic potass, or liquor potassæ of the L. Pharmacopœia, in the dose of twenty or thirty drops, alone, or in combination with the precipitated sulphur, is likewise beneficial; and the tinctura veratri, given in such doses as not to disorder the bowels, has occasionally removed this state of the disease.

"When the skin is highly inflamed, thickened, and stiff, of a vivid red color, intermixed with a yellowish hue, (where the cuticle is separating in large flakes,) the heat, pain, and itching are often extremely troublesome, and the motion of the limbs is almost impracticable. The most effectual relief is obtained, in these cases, by gently besmearing the parts with cream, or a little fresh and well washed lard, or butter.

"3. Lepra *nigricans* is a more rare variety of the disease, differing externally from the L. vulgaris chiefly in the dark and livid hue of its patches, which is most obvious in the margin, but even appears through the thin scales in the area of each patch.* The scales are more easily detached in this form of lepra, and the surface remains longer tender, and is often excoriated, discharging bloody serum, till a new incrustation is formed.

"This variety of lepra occurs in persons whose occupations expose them to the vicissitudes of the weather, and to a precarious diet, with fatigue, and watching. It is cured by nutritive food, with moderate exercise, followed by the use of the bark, mineral acids, and sea-bathing."

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* The *Melas* of the ancients was deemed a superficial affection, resembling the Alphos, except in its color. 'Μελας colore ab hoc differt, quia niger est, et umbræ similis: cætera eadem sunt.' (Celsus, loc. cit.) Possibly it included the Pityriasis versicolor."

MEDICAL

MEDICAL AND PHILOSOPHICAL INTELLIGENCE.

ROYAL SOCIETY.—On Thursday the 1st of July, a paper by Sir Humphry Davy was read, containing farther observations on the new detonating compound of chlorine and azote. After recovering from the accident which happened to him during his original experiments on this substance, Sir Humphry made a variety of experiments to determine its properties and composition. Its specific gravity is 1·623. When in contact with water, it congeals at about 40°; but this does not happen when it is kept separate from water. It detonates in nitric acid, and in ammonia; gives out azote in muriatic acid; and is likewise decomposed in sulphuric acid. Attempts were made to decompose it in exhausted glass vessels in the state of vapor; but they were unsuccessful. In general, the glass was broken by an explosion; and when that did not happen, the proportion of chlorine and azote evolved could not be determined, on account of the unknown proportion of atmospheric air that remained in the vessel. When the detonating compound is brought in contact with mercury, a white powder is formed, and azotic gas disengaged. In one experiment a detonation took place, which obliged him to work upon smaller quantities. The white powder was found to be a mixture of calomel and corrosive sublimate; and it sublimed entirely without the disengagement of any gas, indicating the absence both of hydrogen and oxygen. Muriatic acid does not destroy the color of solution of indigo in sulphuric acid; but if it be impregnated with chlorine, it destroys a determinate quantity of the blue color, according to the proportion of chlorine present. The same thing happens when the detonating compound is dissolved in muriatic acid. This furnished a method for determining the proportion of chlorine contained in the detonating compound. The result of all these methods of analysis is, that the detonating compound is composed of

Chlorine	91
Azote	9
	100

reckoning by weight, or if we reckon by bulks of

Chlorine	400
Azotic gas	100
	500

Sir H. Davy proposes to call this detonating compound *azotane.*

At the same meeting were read some observations on a new comet, observed by Captain Hill, in the Hon. East India Company's service.

On Thursday, the 8th of July, the following papers were read:

1. A catalogue of the positions of a number of circumpolar stars by the Astronomer Royal, Mr. Pond.

2. An analysis of a substance thrown out of Mount Vesuvius, by

 James

James Smithson, Esq. This substance had been sent to Mr. Smithson when in Italy in 1794, in order to determine its nature; and he ascertained, by a number of trials, that it consisted chiefly of sulphate of potash. This result was published soon after in an Italian Journal, but no subsequent notice was taken of it by mineralogists. Mr. Smithson was induced to examine it with more accuracy lately, and the result of his experiments is, that it consists of sulphate of potash, sulphate of soda, sulphate of ammonia, muriate of ammonia, muriate of copper, and muriate of iron, with some earthy matter. Mr. Smithson, by way of introduction to his paper, gives a view of his opinions about the origin of the earth. In his opinion, it was originally a sun, or a comet, and was brought to the state in which it is at present by undergoing combustion at the surface. The volcanoes are the relics of this original combustion, and the materials were the metallic bases of the earthy substances of which the primitive strata are composed. As a proof that these primitive strata have been formed by combustion, he mentions that garnets, hornblende, and other crystals found in them, contain no water, and that little or no water is to be found in the primitive strata themselves.

3. Observations, by Dr. Marcet, on the cold produced by the evaporation of sulphuret of carbon. This liquid evaporates more rapidly than any other, and produces in consequence a greater degree of cold. A spirit of wine thermometer, having its bulb surrounded with cotton cloth or lint, if dipped into sulphuret of carbon, and suspended in the air, sinks from 60° to 0. If it be put into the receiver of an air-pump, and a moderate exhaustion be made, it sinks from 60° to — 81° (I have seen it myself in these circumstances sink from 74° to — 72°). If a tube containing mercury be treated in the same way, the mercury may be readily frozen, even in summer. The drier the air in the receiver is, the more easily is the cold produced. Hence the presence of sulphuric acid may be of some little service in removing the vapor from the air in the receiver previous to exhaustion; otherwise it occasions no increase of the cold.

4. Observations on the composition of fluor spar, and on its acid basis, by Sir Humphry Davy. The author begins his paper with an historical detail of the attempts made by himself, and Gay-Lussac and Thenard, to decompose fluoric acid, and the ill success of these attempts. It appears from the compounds into which fluoric acid enters, that the weight of an integrant particle of it does not exceed 1·05, supposing an atom of oxygen to weigh 1. Hence it follows that if it is a compound of oxygen and an inflammable base, the base can only have the 20th part of the weight of the oxygen. This supposition he considers as unlikely to be correct. He therefore supposes that fluoric acid, like muriatic acid, is a compound of hydrogen and an unknown supporter of combustion, to which he gives the name of fluorine. He relates many experiments made in order to obtain fluorine in a separate state, but none of them were attended with success. As chlorine has the property of decomposing several oxides, and driving off their oxygen, it occurred to him as likely that it might in certain cases

cases drive off fluorine. Fluate of silver and fluate of mercury, with this view, were acted upon by chlorine. The fluoric acid (or fluorine) was disengaged, and horn silver and corrosive sublimate formed; but no fluorine was set free. When the experiments were made in glass retorts, the glass was corroded, and silicated fluoric acid gas obtained. When they were made in platinum vessels, the metal was corroded, and a red or brown powder formed. It would seem from the trials made that fluorine has so violent an action on all other bodies that it is very difficult, if not impossible, to obtain it in a separate state. The author promises to continue this subject in a subsequent paper.

5. A paper, by Mr. Alman, on the method of freeing equations from surds, though the roots be pretty high in their dimensions. From the nature of this paper it could not be read at full length, so that it is not possible to give any idea of the method employed by the author; but he referred to a method previously given in the Irish Transactions by Mr. Money, which he considers as general, and which, probably, constitutes the foundation of his own.

The Society adjourned till the 4th of November next.

Galvanic Battery.—On Saturday, the 2d of July, J. G. Children, esq. put in action the greatest galvanic battery that has ever been constructed. It consisted of 20 pair of copper and zinc plates, each plate 6 feet in length, 2 feet 8 inches in breadth. Each pair was fixed together at the top by pieces of lead cut into ribbons. A separate wooden cell was constructed for each pair. The plates were suspended from a wooden beam fixed at the ceiling, and were so hung by means of counterpoises that they could be easily raised or let down into the cells. The cells were filled with water, containing a mixture of sulphuric and nitric acids. At first the acids amounted to 1-60th of the water; but more was gradually added till it amounted to the 30th. Leaden pipes were attached to the two extremities of the battery, and conveyed the electricity out of doors to an adjoining shade, where the experiments were made. The power of this battery was very great; though I am not certain whether it increased in proportion to the size of the plates. It ignited about 6 feet in length of thick platinum wire. The heat produced was very intense. It melted platinum with great facility. Iridium was likewise melted into a globule, and proved to be a brittle metal. The ore of iridium and osmium was likewise melted, but not so completely. Charcoal was kept in a white heat in chlorine gas, and in phosgene gas; but no change took place in either of these gases. Neither tungsten nor uranium underwent any change. A very singular fact was pointed out by the sagacity of Dr. Wollaston, and succeeded upon trial. A greater length of thick platinum wire was ignited than of platinum wire of a much smaller size. This Dr. Wollaston had previously ascertained in his own minute galvanic batteries, consisting of a single pair of small plates.

Volatility of Cerium.—The volatility of this metal, which had been previously inferred from the experiments of Vauquelin, was fully confirmed in Mr. Children's laboratory at Tonbridge. A quantity

tity of oxalate of cerium had been prepared in order to obtain from it the oxide of the metal. This oxalate was exposed in a charcoal crucible within a tobacco-pipe mouth to the strongest heat that could be raised in a forge. In this heat the cerium was volatilized so completely that not the least trace of it remained.

Action of the Agate on Light.—Dr. Brewster has now established, by numerous experiments, that the nebulous light of the agate has the same relation to the bright image as the extraordinarily refracted image has to the ordinarily refracted image of all doubly refracting crystals. There is still, however, no appearance of the nebulous light being produced by a higher refractive power than the bright image. All the phenomena of polarization are produced when the plate of agate is less than the $\frac{1}{150}$th of an inch; and the nebulous light, when in its evanescent state, can be revived by depolarization with mica, in every respect like one of the images formed by double refraction. The colored appearances in the agate, to which mineralogists have given the name of iridescence, furnish a series of the most singular results, arising, apparently, from the mechanical structure of different laminæ, and seem to open up a new field in physical optics. He is at present examining various specimens, in order to give a greater degree of generality to the results previous to laying them before the public.

An Account of the Explosion of Inflammable Air which lately occurred in the Collingwood Main Colliery.

On Saturday the 17th of July, at two o'clock, p. m. in the Collingwood Main Colliery, situated upon the river Tyne, near North Shields, a very considerable quantity of inflammable air, or carbureted hydrogen gas, came into contact with the pitmen's candles, which caused a most tremendous explosion, by which eight persons were killed upon the spot, and two severely wounded and scorched. The following particulars of this melancholy disaster were communicated verbatim at the above-mentioned colliery, a few days after the accident, to the writer of this, by Henry Hall, who fortunately escaped, though in the midst of imminent danger.

At the time when the explosion took place, the above-named Henry Hall, and five other pitmen, were proceeding with burthens of timber through the old workings or excavations (the proper road being obstructed by a creep,*) in the full confidence of safety, having been assured by Mr. Hope, the under viewer, that there was no fear of the "mine firing." In an instant this young man, Henry Hall, and the five pitmen who were with him, were by the explosion thrown upon their faces; and the shock was so great as to deprive him of sensation, as well as volition, till the after-blast, or after-damp, as it

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* In working the coal, the pitmen leave pillars, in the form of parallelograms, for the support of the roof. If these pillars are narrow, and the floor of the mine soft or tender, they are apt to sink into the floor, and cause such an approximation as to prevent ventilation, &c. This is technically called *a creep*.

is called, gave him such excitement that he faintly recollects being urged like a ball along the floor of the mine with incredible velocity. Soon after this he was again deprived of sensation, in which state he continued for about twenty minutes, till he breathed the pure atmospheric air upon the bank, at the top of the shaft, to which place his brother had carried him, who descended into the mine as soon as he possibly could, upon hearing the explosion, at the risk of his own life, for the purpose of saving that of his brother, or of any other person whom he could find. H. Hall reports, that after he recovered sensation he felt his whole body racked with pain, the burnt places giving him no uneasiness, comparatively speaking; and that his suffering continued without intermission for two days. Bad as H. Hall's case was, the other five pitmen who were with him had not even such an escape, for four of them were instantly killed, and Ralph Stokell so dangerously bruised and burnt in several places, that his life was for some time despaired of.

At a distant part of the mine, where some other pitmen were employed in taking up metal plates, timber, &c. Mr. Hope, the under viewer, Mr. Wild, the overman, and two pitmen, were suffocated by the choak-damp, or carbonic acid gas. Mr. Wild had wandered at least a hundred yards before he met his death by suffocation.

Upon an explosion taking place in a coal-mine the choak-damp is very rapidly driven through all parts of the colliery from those places where it had accumulated, and the explosion is always followed by another commotion, of a still more dangerous nature, viz. the "back draught," as the miners term it. The back draught is that impetuous current of air which rushes most violently from all sides within the mine, like "the voice of mighty thunderings," to the spot where the explosion occurred, so as to overcome the vacuum which had been effected by means of the explosion.

The following is a list of the persons who were killed:—

Mr. William Hope, under viewer, leaving a wife and four children.

Mr. Ralph Wild, overman, a wife and four children.

James Campbell, pitman, a wife and child.

Ralph Hope, pitman.

Robert Clark, pitman.

Thomas Miller, pitman.

George Richardson, and William Richardson, pitmen. These two young men were brothers; and, having lost their parents, they had the filial goodness to support their grandmother, now in her 103d year, by their industry.

By the choak-damp a considerable number of horses were suffocated. In this melancholy list the dreadfully uncertain state of the pitmen is clearly demonstrated. Poor Mr. Hope, the under viewer, was heard to exclaim, in astonishment or despair, a moment before his dissolution, "God have mercy upon us; the pit has fired!" Besides the sufferers, there were fourteen or fifteen men in the pit, who, as if by a miracle, were saved. They had been employed in a distant part of the colliery; and after the explosion wandered on in darkness and stupefaction till by good fortune they chanced to arrive at that part of the mine where there was a sufficient proportion of atmospheric air to support respiration.—*Annals of Philosophy.*

IMPERIAL

IMPERIAL INSTITUTE OF FRANCE.

Account of the Transactions of the Imperial Institute of France for 1812.

(*Continued from p. 256.*)

Zoology, Anatomy, and Animal Physiology.—M. le Chevalier Geoffroy-Saint-Hilaire, who has examined at various intervals the numerous family of bats, and has made us acquainted with many interesting species, proposes to give a general table of them. He has prefaced this undertaking with a dissertation on the rank which these singular animals ought to hold among the mammalia. They were long considered as intermediate between quadrupeds and birds. It is equally obvious that they hold an intermediate place between the quadrumania and carnivorous animals. Among the numerous arrangements proposed by naturalists, there are some, as that of Linnæus in his last editions, and that of Brisson, in which the bats are classed along with the quadrumania; in others, as that of Linnæus in his first editions, and that of Klein, they are placed with the small carnivorous animals, or eaters of insects, as the mole and the hedgehog. Some, as Storr and Cuvier, place them at the head of carnivorous animals, before the insect eaters just mentioned, and immediately after the quadrumania; with this difference, however, that Cuvier distinguishes them more particularly, and makes a subdivision of them. Others, as Ray, Blumenbach, Lacepede, and Iliger, constitute them a separate order; and this order is placed by Ray and by Lacepede in some measure out of the arrangement: by Blumenbach between the quadrumania and the other inguicula, at the head of which this naturalist places the *rongeurs*. Finally, M. Iliger places them before the carnivorous animals, at the head of which are placed, as in the arrangement of Cuvier, the devourers of insects.

It is easy to see that all these combinations will depend upon those organs to which each naturalist has paid the greatest attention. Those who have chiefly attended to the skeleton, to the intestines, to the organization of the feet, to the form of the nails, to the grinders, have considered the bats as analogous to carnivorous animals (and this is the opinion at present most followed); while those who have attended only to the fore-teeth, to the position of the mammæ, to the hanging penis, have considered them as analogous to the quadrumania.

M. Geoffroy, in the work of which we have spoken, insists more than usual upon these last relations, to which he thinks sufficient attention has not been paid. He shows particularly that the singular elongation of the anterior extremities, the general tendency of the skin to become excessively wide, and the peculiar properties which are the consequence of this in the bats, both with respect to their sensations and motions, require us to place these mammalia in a separate order; while, at the same time, their striking resemblance to the quadrumania, and to the carnivorous animals, requires that this order should be placed between them. We may look with interest to the subdivision of this order, and to the history of the species, which M. Geoffroy has promised.

M. de

M. de la Mark, employed at the Museum of Natural History in teaching every thing which concerns the animals destitute of vertebræ, published, some years ago, the work which serves as a basis to his course. He explains in it, according to his own method, the classes, orders, and genera, of these numerous animals: but as travellers have since discovered many genera and species, as anatomists have more completely explained the structure, and as the meditations of la Mark on the subject have made him discover various new relations among these animals, he has published an abridged table of his course, after his method in its most perfect state, in which he satisfies himself with giving the characters of the greater divisions, and simply enumerating the names of the genera.

He follows in their arrangement the degrees of complicateness, beginning with the most simple animals. Supposing that those which have no visible nerves only move in consequence of their irritability, he calls them *apathic animals*. He gives the names of *sensible animals* to the other animals without vertebræ, and of *intelligent animals* to those which have vertebræ. To his old classes, now well known to naturalists, he adds the *cirrhipeda*, which include the *glands-de-mer*, and other analogous animals, and which he places between his annelides and his mollusca; that of the *epizoaines*, or intestinal worms, which he places among his apathic animals; and the *infusoria*, or microscopic animals without visible mouth or intestines. He leaves the *echinodermes* in his *radiaires*, and among the apathic animals, with a greater degree of simplicity than the intestinal worms. We regret that we have not room to notice the other changes introduced by M. de la Mark into his orders, nor the numerous additions which he has made to the list of genera; but naturalists will not fail to look for them in the work itself.

Notwithstanding the success of the anatomical investigations of animals without vertebræ for several years back, there still remained a family in which the fundamental organs were not well known. It is the family called *echinodermes*, which comprehends the star-fish, and other analogous genera. The Class having proposed a prize for the perfecting of this branch of comparative anatomy, it was gained by M. Tiedeman, Professor in the University of Landshut. The memoir of this skilful anatomist makes us accurately acquainted, for the first time, with many particulars respecting the organization of these singular animals. A species of circulation is easily observed between their organs of digestion and those of respiration, without, however, offering a complete double circle. Nor can the branches be followed in the exterior organs, nor in those of motion. It appears even, according to M. Tiedeman, that a quite different vascular system is distributed to those numerous peduncles which in these animals serve for instruments of locomotion.

The organs of respiration differ much in different genera. In the holothuria they represent hollow trees, whose branches fill and empty themselves with water from without, and are interlaced with a vascular net. In the *stars* and urchins the water penetrates immediately into the cavity of the body, and moistens all the parts of it.

This

This beautiful work, accompanied by plates exquisitely finished by M. Münz, Doctor of Medicine, appeared to the Class to deserve the prize, by the number of new facts well authenticated which it presents, and by the great progress which it has made to the intimate knowledge of the echinodermes, though it has not completely answered the question proposed relative to their circulation.

A family much more simple in its organization than the echinodermes, but much more numerous in species, namely, the *corals*, and other animals composed of a solid basis, has been particularly studied by M. Lamouroux, both with respect to the species and the methodical arrangement. This naturalist has made a great collection of those whose basis is not stony, and which present forms so agreeable, and often so singular; and comparing with much care the form, the mutual position, of the cells from which the polypi issue, and all the other visible differences of these animals, he proposes to add twenty-eight new genera. This is an important work for completing the system of animals; but it does not, from its nature, admit of an abridged analysis. We are anxious for its speedy publication.

M. Cuvier, proposing soon to begin printing the great book on comparative anatomy with which he has been occupied for so many years, has presented to the Class a table of the divisions according to which the animal kingdom will be distributed in that work. Naturalists have been long struck with the great differences which separate animals without vertebræ from each other, while animals with vertebræ resemble each other in so many respects. Hence a great difficulty in generalizing that branch of comparative anatomy, while it is easy to generalize what relates to the animals with vertebræ. But this difficulty has suggested its own remedy. From the manner in which the propositions relative to each organ are always grouped, M. Cuvier concludes that there exist among animals four principal forms. The first is that which is known under the name of animals with vertebræ; and the three others are nearly similar to it in the uniformity of their respective plans. The author calls them *mollusca, articulated animals*, and *radiated animals*, or *zoophites*. He subdivides each of these forms or branches into four classes, from motives nearly similar to those which have produced the four classes generally adopted among the animals with vertebræ. He has drawn from this disposition, in some respects symmetrical, a great facility in reducing under general rules the differences of organization.

The comparison which the same author has made of the osteology of the animals with vertebræ, has given him ideas respecting the bony structure of the heads in this class, which he has likewise presented to the Class.

It had been for some time observed that the oviparous animals with vertebræ, that is to say, birds, reptiles, and fishes, had certain common relations in their structure which distinguished them from the mammalia. M. Geoffroy-Saint-Hilaire had even presented some years ago an elaborate essay on the subject, of which an account was formerly given, in which, among other things, he had shown the

 identity

identity of the structure of the heads of oviparous animals, and the resemblance in the numerous pieces which enter into their composition, with that of those which we distinguish in the fœtus of mammalia, where, as is known, the bones are much more subdivided than in adults.

M. Cuvier, adopting the views of M. Geoffroy, has tried to determine in an accurate manner to what bone of the head of mammalia corresponds each group of bones in the head of the different oviparous animals; and he conceives he has succeeded, by joining to the analogy of the fœtus of the first the consideration of the position and of the functions of the bones; that is to say, by examining what organs they protect, to what nerves and vessels they give passage, and to what muscles they furnish attachments.

M. Jacobsen, Surgeon-Major in the armies of the King of Denmark, has made known to the Class an organ which he discovered in the nostrils of quadrupeds, with which no anatomist seems to have been acquainted. It consists in a narrow sack placed along the canal of the nose, defended by a cartilaginous production, covered internally by a mucous membrane, doubled in part by a glandular tissue, receiving remarkable nerves, with distinct divisions of the first pair, and which open most commonly into the palate, behind the fore-teeth, by a canal which passes through the hole called *incisive* by anatomists. This organ does not exist in man, and is more distinct in most herbivorous animals than in the carnivorous. We must suppose that it is connected with some of those faculties which nature has given to quadrupeds, but denied to our species, as that of rejecting poisonous substances, of distinguishing the sex, the state of heat, &c.

The particular history of animals is enriched with important works and interesting observations.

M. de Humboldt, Foreign Associate, has published the first volume of his *Observations on the Animals of America*, in which he has inserted not only his different researches on the condor, the electric eel, the crocodile, and many other objects of which we have spoken in our preceding analyses; but he has likewise given several new memoirs, namely, on the apes of the New World, of which Buffon and Gmelin only made known 11 or 12 species, but which Humboldt, uniting his observations with those of Azzara and Geoffroy-Saint-Hilaire, makes 46. He has recently read to the Class another memoir intended for his second volume, in which he describes new species of serpents that he found in Guyana.

The tempests which agitated the sea last winter threw ashore several large cetaceous fish upon our coasts. The Class appointed, as a commission to examine the facts which were received respecting these animals, MM. le Comte Lacepede, Geoffroy-Saint-Hilaire, and Cuvier.

These naturalists have observed that several of these animals were formerly unknown, and that this subject, which might be interesting to our fisheries and our commerce, deserved to draw the attention of government. They gave a description of a species thrown ashore in

great

great numbers near St. Brieux. M. Lemaout, naturalist and apothecary in that city, having collected with much care all the essential parts, it was easy to recognise a species of dolphin hitherto unknown to naturalists, and of which there only existed a bad figure in the treatise on fishes by Duhamel. It is distinguished by the globular form of the head, almost similar to an ancient helmet. Its length is about 20 feet.

We noticed last year the researches of M. Lamouroux on the innumerable and very small eels, known at the mouths of some of our rivers by the name of *montée*, and we announced the probability that they might belong to some of the little known species of this genus. M. Lamouroux has determined by new experiments that the montée is the fry of the *pimperneau*, a species of eel noticed by Lacepede in his history of fishes, and which is distinguished from the others by its pectoral fins being hollowed out like the wings of bats.

Our associate M. de la Billardiere, who employs himself in bringing up bees, having observed one whose abdomen was larger than usual, found in it a white worm, which M. Bosc examined. The body of this worm was white, divided into twelve rings, flattened below, terminated at one extremity by two large tubercles, pierced each with an oval hole, and at the other by two soft points. Under the tubercles is a transverse slit. M. Bosc, regarding this slit as the mouth, considers the part terminated by two points as that where ought to be the anus; and ranging the animal among intestinal worms, he makes a new genus of it, under the name of *dipodium*. He admits, however, that it is possible that the organs may in fact be reversed, and then the worm would much resemble the larvæ of flies with two wings. There is reason to believe, from the observations of Latreille, that the larva of one of these flies, (the *canops ferruginosa*) lives in the interior of the drones. It is very remarkable that so large a worm should inhabit the body of an insect so small as a bee.

The portion of digestion which takes place in the stomach must have early attracted the attention of physiologists, and recourse was had successively to all the powers of nature to explain it. It was long ascribed to the muscular trituration of the stomach; but Reamur having remarked that food contained in incompressible tubes, open at the two ends, was digested like other food, the general opinion since that time has been that the food was dissolved by means of a juice secreted by the stomach.

The Committee who undertook to make enquiry into, and ascertain the extent of the Process practised by Messrs. Delahoyde and Lucett for the Relief of Persons afflicted with Insanity, and to provide the means of paying the expense of such enquiry, have published their first Report.* It consisted of the Duke of Kent, the Duke of Sussex, Lord Dundas, Earl Fitzwilliam, Lord Milton, Viscount Melville, the Duke of Bedford, the Earl of Cork, and seven

* Chapple, Pall-Mall, 1s.

other gentlemen. Being informed by Dr. Harness that the government receiving-house at Hoxton contained the greatest number of the most confirmed lunatics, and having procured a list of them, the Committee on the 7th of June held their meeting at the said receiving-house. They then made an arrangement with Messrs. Delahoyde and Lucett, as to terms for receiving, in the first instance, *four patients* into their care, and afterwards such further number as might be thought proper.

On the 27th of June, *John Braily*, being one of the selected list, and who appeared to the Committee to be more violent if possible than the rest, heavily chained and handcuffed, with an horse-cloth, by way of covering, about his waist, was conducted to Sion Vale. Braily, by the list, was described as coming from his Majesty's ship the Phœnix, and it appeared that he had been received at Hoxton on the 18th of September, 1809, from whence, after remaining there twelve days, he had been removed to Bethlem Hospital, and there continued a year and eleven days, when he was, on the 11th of October, 1810, returned to Hoxton as an incurable maniac, and there remained till he was removed to Sion Vale.

William Matters had, it appears by the list, been a marine on-board the Revenge, and was sent to Hoxton the 16th of April, 1808, and after remaining there seven days, was sent to Bethlem Hospital, where he continued one year one month and seven days, and then returned as incurable to Hoxton, and, after remaining there four years and two months, was conducted to Sion Vale, viz. on the 30th of July.

James Cardiff, formerly a marine on-board the Sussex, was, as appears by the list, sent to Hoxton the 22d of February, 1810; and from thence, at the end of a month and twenty days, to Bethlem, where he remained a year and ten days, and was then returned to Hoxton, and continued there two years three months and six days previous to his removal to Sion Vale, which was on the said 30th of July.

Daniel O'Keefe had been quarter-master-serjeant in the 47th regiment, and confined at Hoxton, from whence he was conveyed to Bethlem, and in October, 1812, discharged as incurable, as appears by a letter from the War Office, to John Ridge, esq. the agent to the regiment, dated the 14th of October, 1812; and was discharged from the service by reason of his having been reported incurably deranged in his intellect; as appears by a letter to Mr. Ridge, dated Horse Guards, 21st of November, 1812, signed Harry Calvert. O'Keefe was then put upon the out-pension list of Chelsea Hospital, by an order dispensing with his personal appearance before the board, and being under the care of his wife at Thatcham, was by her consent removed to Brentford, and after being examined by their royal highnesses the Dukes of Kent and Sussex and Dr. Harness, was placed at Sion Vale under the care of Mr. Delahoyde.

At a meeting of the Committee, on the 7th of August, Dr. Harness was requested to take the trouble to inspect the state of these four patients at Sion Vale, which was accordingly done, as appears by his Report, dated the 27th of August, 1813.

"Upon the 10th of August, 1813, I visited the patients at Sion Vale,

Vale, accompanied by Dr. Weir, Naval Medical Inspector of Hospitals, when we found *Cardiff* assisting in brewing beer for the use of the family—continued in conversation with him some minutes, after which Dr. Weir, who had been in the habit of seeing him almost weekly during his confinement at Hoxton, pronounced him to be in a better state than he recollected ever to have seen him. Dr. Weir observed that he had frequently seen Cardiff employed in the service of the family at Hoxton.

"We afterwards saw *Braily* walking in the back garden, but I am sorry to observe not in the quiet composed state I had been accustomed to find him on my former visits; he being particularly talkative, complaining of his situation, and being constantly annoyed by numerous people, mostly women, relating their complaints to and of him; upon which I asked him, if he felt himself better than when at Hoxton; to which he replied that he certainly did; but that he did not know how long he might continue so, as he found his head becoming much confused, which observation occasioned Mr. Delahoyde to remark that he should again undergo the curative process in the evening. On Saturday the 21st instant, Mr. Delahoyde called on me at the Transport Office, to inform me that Braily had undergone the curative process on the evening of the 10th instant, and that he had derived considerable benefit therefrom.

"Upon the 25th, I visited Sion Vale, when I found Braily much more calm and collected than when seen by Dr. Weir and myself upon the 10th instant. Conversed and walked in the garden with him more than a quarter of an hour, during which time he conducted himself with great propriety, but occasionally expressed himself with more than usual rapidity, but in language strictly decorous.

"Upon the 10th instant saw *O'Keefe* and *Matters* working in the garden; they both appeared composed and reconciled to their situation. Dr. Weir thought *Matters* improved.

"Each of them, after being talked to a short time, reverted to his peculiar complaint, viz. O'Keefe to the loss of his hair, and Matters to his being distended by air.

"On my visit of the 25th instant, I could not discover that *O'Keefe* had made any progress to amendment.

"*Cardiff* and *Matters* I consider improved; they both bearing me to converse with them upon those subjects on which they had rested their principal complaints; and which subjects could not before have been touched upon without exciting considerable emotion.

"J. Harness."

The following is a copy of the minutes of the Committee, held at Sion Vale, the 15th of September, 1813, present the Duke of Kent, Thomas Smith, Esq. and Dr. Harness.

"The Committee now assembled, having conversed with and examined the four public patients at this place, with a view to form their judgment of them, came to the following opinion.

"Braily, certainly more calm and collected than on the 25th ultimo, or at any time when seen by Dr. Harness since his arrival at Sion Vale.

"O'Keefe, perfectly calm, and his general health improved, but recollection still much impaired.

 "Cardiff,

"Cardiff, much better, but does not appear wholly free from the influence of the particular cause to which he attributes his complaint.

"Matters does not appear to labor under the least symptoms of complaint."

The process itself is reserved by them from the public; but having been detailed by Mr. TARDY, of Marchmont Stuart, in our Number for August last, we consider these cases as mere exemplications of his mode of treatment, and in that point of view they merit the notice of our readers.

Philosophical Society of London.—On Wednesday, October 6th, the anniversary of this Society was celebrated. The following gentlemen were elected officers and council for the year ensuing:—

President.—J. C. Lettsom, M. and LL.D. F.R.S. F.A.S. &c. &c.

Vice-Presidents.—J. Taunton, F.A.S., J. J. B. Beaumont, F.A.S., George Rees, M.D., J. Adams, M.D. F.L.S., J. Sowerby, F.L.S. G.S. W.S. &c., J. Hamilton, M.D.

Treasurer and Secretary.—T. J. Pettigrew, F.L.S.

Curators.—W. C. Pettigrew, J. Hare, junior.

Registrar.—J. Meers.

Orator.—Dr. George Rees.

Council.—W. Bullock, F.L.S., T. Bedder, J. Hare, R. Thompson, J. B. Brown, B. Clarkson, J. P. Heath, F. K. Cromwell, W. Henley, H. Patten, C. Holloway, W. Clarke, J. Collins, J. Andrewes, T. Lewis, R. Saumarez, T. Bittlestone, J. Wright.

The Anniversary Oration was delivered at half-past three o'clock, by Dr. Lettsom, which it is expected will be published.

Surrey Institution.—The following arrangements have been made for Lectures in the ensuing season:—Mr. J. Mason Good on the Philosophy of Physics, to commence on Friday the 5th of November, and to be continued on each succeeding Friday; Dr. Thompson on Chemistry, to commence on Tuesday, November 9, and to be continued on each succeeding Tuesday; and Mr. Bakewell on Natural and Experimental Philosophy, to commence early in January 1814.

The excellent Society for the Relief of Widows and Orphans of Medical Men in London and its vicinity, held its Anniversary Festival for 1813, on the 29th of October, and was numerously and most respectably attended.

The income of the Society for the last twelve months, ending the 15th of September, was as follows:

By subscriptions of members and donations	£325	8	0
From funded property	601	9	0
	£926	17	0

In the course of last year, the treasurer has been enabled to purchase 2000*l.* stock, three per cent. consols; which brings the present capital of the Society to 24,400*l.* three per cent consols, producing the annual sum of 612*l.*, to which stock an increase is made every half year; it possesses also 200*l.* navy five per cents, producing annually 10*l.*; total annual dividends at present 622*l.*

The sum distributed among widows and orphans during the present year has been 357*l.* 10*s.*

The number of members belonging to the Society is 295.

LONDON

LONDON PRICES OF DRUGS.—Oct. 22.

(To be continued regularly.)

	£.	s.	d.	£.	s.	d.	per
Aloes Barbadoes, from	21	0	0 to	22	0	0	C.
——— Cape	9	10	0 ..	10	0	0	—
——— Succotrina	20	0	0 ..	0	0	0	—
——— Epatica or E.I.	10	0	0 ..	12	0	0	—
Angelica Root	9	10	0 ..	10	10	0	—
Alkanet Root	9	5	0 ..	9	10	0	—
Antimony Crude	3	15	0 ..	4	5	0	—
Aqua Fortis	S.	0	8 ..	D.	1	1	lb
Arrow Root fine	0	2	0 ..	0	3	6	—
——— ordinary	0	1	0 ..	0	1	6	—
Arsenic Red	6	10	0 ..	6	15	0	C.
——— White	3	12	0 ..	3	15	0	—
Balsam Capivi	0	5	0 ..	0	5	6	lb.
——— Peru	0	15	0 ..	0	16	0	—
——— Tolu	0	12	0 ..	0	0	0	—
Bark Jesuits Com.	0	2	0 ..	0	3	0	—
——— Second	0	4	0 ..	0	5	0	—
——— Quil or best	0	7	0 ..	0	8	0	—
——— Red	0	7	0 ..	0	10	0	—
——— Yellow	0	3	0 ..	0	4	6	—
Borax refined E.I.	5	10	0 ..	6	0	0	—
——— English	0	2	8 ..	0	0	0	lb.
——— unrefined or Tinc.	6	0	0 ..	0	0	0	C.
Camphire refined	0	6	9 ..	0	0	0	lb.
——— unrefined	24	0	0 ..	25	0	0	C.
Cantharides	0	12	3 ..	0	12	6	—
Cardamoms (best)	0	7	6 ..	0	0	0	lb.
Cassia Buds	12	0	0 .. }		0	0	C.
——— Fistula W.I.	12	0	0 .. }	in bond			—
——— Lignia	12	0	0 .. }		0	0	—
Castorum American	2	8	0 ..	2	10	0	lb.
——— Russia	11	0	0 ..	0	0	0	—
Castor Oil per bottle 1½ lb.	0	3	9 ..	0	4	3	bo
Coculus Indicus	3	0	0 ..	0	0	0	C.
Colocynth Turkey	0	4	6 ..	0	0	0	lb.
Columbo Root	2	15	0 ..	3	0	0	C.
Cream of Tartar	8	10	0 ..	9	10	0	—
Gallangal East-India	5	0	0 ..	0	0	0	C.
Gentian Root	4	12	6 ..	0	0	0	—
Ginsang	0	1	6 ..	0	0	0	lb.
Grains of Guinea	5	10	0 ..	6	0	0	C.
Gum Ammo. Drop	15	0	0 ..	20	0	0	—
——— Lump	8	0	0 ..	9	0	0	—
——— Animi	3	10	0 ..	7	10	0	—
——— Arabic E.I.	2	0	0 ..	4	0	0	—
——— Turkey Fine	8	10	0 ..	9	9	0	—
——— Barbary	3	10	0 ..	3	12	6	—
——— Assafœtida	15	0	0 ..	18	0	0	—
——— Benjamin	8	10	0 ..	35	0	0	—
——— Cambogium	20	0	0 ..	26	0	0	—
——— Copal scraped	0	2	0 ..	0	3	6	lb.
——— Galbanum	15	0	0 ..	16	0	0	C.

	£.	s.	d.	£.	s.
Gum Guaiacum, from	0	2	6 to	0	4
——— Mastic	0	4	6 ..	0	0
——— Myrrh	15	0	0 ..	18	0
——— Olibanum	8	0	0 ..	10	0
——— Oppoponax	80	0	0 ..	0	0
——— Sandrac	6	10	0 ..	7	7
——— Seneca Garbled	5	2	0 ..	5	5
——— Tragacanth	48	0	0 ..	50	0
Jalap	0	4	0 ..	0	0
Ipecacuanha	0	14	0 ..	0	14
Isinglass Book	0	11	6 ..	0	12
——— Leaf	0	11	6 ..	0	12
——— Long Staple	0	11	6 ..	0	12
——— Short Staple	0	11	6 ..	0	12
Manna Flakey	0	8	0 ..	0	8
——— Sicily in Sorts	0	5	0 ..	0	0
Musk China	0	17	0 ..	0	18
Nux Vomica	2	0	0 ..	2	2
Oil of Vitriol	0	0	3¼	0	0
Opium East-India	0	19	6 ..	1	0
——— Turkey	1	2	0 ..	0	0
Pink Root	0	5	0 ..	0	0
Quicksilver	0	4	11 ..	0	0
Rhubarb East-India	0	3	0 ..	0	5
——— Russia	0	14	0 ..	0	0
Saffron Spanish	1	10	0 ..	1	15
——— French	1	10	0 ..	0	0
Sago	3	0	0 ..	3	5
Sal. Ammoniac	10	10	0 ..	0	0
Sarsaparilla	0	3	0 ..	0	0
Sassafras	10	0	0 ..	0	0
Scammony Aleppo	1	7	0 ..	0	0
——— Smyrna	0	13	0 ..	0	0
Senna	0	2	6 ..	0	4
Seeds Anni Alicant	6	6	0 ..	7	0
——— Coriander English	1	10	0 ..	1	16
——— Cummin	5	5	0 ..	5	10
——— Fenugreek	2	5	0 ..	2	10
Shellack	6	10	0 ..	7	[illegible]
Sticklack	6	10	0 ..	7	[illegible]
Snake Root	0	16	0 ..	0	[illegible]
Soap Castile or Spanish	10	0	0 ..	11	[illegible]
Spermaceti refined	0	2	7 ..	0	[illegible]
Tamarinds West-India	8	10	0 ..	9	[illegible]
Tapioca Lisbon	0	0	8 ..	0	[illegible]
Turmeric Bengal	3	3	0 ..	0	[illegible]
——— China	4	5	0 ..	4	[illegible]
——— West-India	6	10	0 ..	7	[illegible]
Verdigris Wet	0	3	6 ..	0	
——— Dry	0	5	10 ..	0	
——— Crystallized	0	8	6 ..	0	
Vitriol Roman	0	0	7 ..	0	
——— Foreign white	3	10	0 ..	3	[illegible]

Price of Vials per Gross.—8 oz. 70s.—6 oz. 58s.—4 oz. 47s.—3 oz. 43s.—2 oz. 36s.—1 [illegible] ½ oz. 24s.

OBSERVATIONS.

At recent Sales of Merchandize by Auction, *Messrs. Pistor and Co.* sold Canister Fo[illegible] tract Bark, 2s. 8d. per lb.—38 Chests E. I. Gum Arabic, 66s. to 71s. 6d. per Cwt.—— *Griffin and Son.*—14 Chests Red Bark, 3s. 3d. to 3s. 5d. per lb.—11 Chests of Pale Bark[illegible] 1s. 3d. to 5s. per lb.—33 Ferons Yellow Bark, in bond, 2s. 3d. to 3s. per lb.—2 Ch[illegible] fœtida, 10*l.* per Cwt.—2 Chests Gum Ammoniac, 11*l.* 15s. per Cwt.—1 Cask Gum, 30s[illegible]

METEOROLOGICAL TABLE.

From September 26, to October 25, 1813.

D.	Therm.			Barom.		Hygrom. Dry—			—Damp.			Winds.	Atmos. Variation.
26	56	62	55	30^{1}	—	8	13	12				E..	C... F... C..
27	55	63	55	30^{1}	30	11	—	—				NE..	F... —.... C..
28	56	60	53	30	^{1}	7	15	12				NE.	Fog.. F.. C... —...
29	55	59	54	30^{2}	—	12	—	15				NE.	Fog .F..C....R. in N
30	55	59	52	30^{2}	^{1}	11	26	19				E..	F.. — ... — ....
1	50	57	53	29^{9}	^{8}	12	—	15				E..	F... — ... C...
2	52	56	54	29^{8}	^{7}	12	—	—				E..	C... R. C...
3	53	58	55	29^{8}	^{9}	12	—	7				E.SE.	F....C..—..R...in N
4	55	57	63	29^{9}	^{7}	2	—	—	—	10	25	SE.	R... — .... — .. C....
5	61	63	61	29^{8}	^{9}				25	15	—	SW..W..	F... C.. F..
6	60	64	62	29^{9}	^{8}				16	12	15	SW.S.	C..—...—..R....inN
7	68	63	58	29^{6}	^{7}				20	35	30	S.	R... — .... — .. F.
8	55	59	60	29^{8}	—				27	32	36	SW...	F... R.. — .. C....
9	58	60	56	29^{5}	^{6}	—	2	—	30	—	6	NW..	C.... F.... — ...
10	55	59	55	29^{5}	^{2}				15	2	14	W..	R... F... C.. R...
11	52	55	51	29	^{6}				13	—	11	W....	R.. F.. — ...
12	48	56	58	29^{8}	^{6}				15	—	22	W..	F...C...R...—....inN
13	57	54	52	29^{4}	^{7}				27	—	16	NW..	C...F..—...R...inN
14	43	46	53	29^{5}	^{7}				21	9	14	NW.	F.. — ... — ....
15	43	49	47	29^{4}	^{3}				19	—	20	SW...	F.. R.. — ... F..
16	44	47	50	29^{3}	28^{8}				20	10	20	SW..SE..	F. R... — ... C....
17	48	53	46	28^{8}	29^{2}				19	—	15	W..NE..	F.. R... — .. F..
18	43	50	43	29^{4}	^{6}				15	—	11	NW..	F... — ... C...
19	39	47	43	29^{7}	—				16	9	16	NE..	F... — ... C..
20	42	48	42	29^{7}	—				16	10	20	NE..	Fog ... C..
21	50	54	51	29^{6}	^{7}				21	30	17	E..	R... — ..
22	52	56	55	30	—				12	17	24	SE..	Fog ... C...
23	55	57	54	29^{9}	30				20	18	20	SE..	Fog ... C.. F.
24	39	48	45	—	—				20	16	15	SE..	Fog .. C..
25	41	54	46	—	—				12	6	10	E.	F.. R. Fog ..

Quantity of rain from the 26th of September to the 25th of October, three ...hes $\frac{60}{100}$. On the 6th, storm of thunder, with heavy rain in the night, ...h, serene lightning, very frequent in the evening. This interval has been ...usually wet for the season of year. The equinoctial gales, which had ... appeared at the accustomed time, have blown with considerable violence ...October.

...nflammatory affections of the chest, especially in children, have been ...quent; and Scarlatina has spread rapidly in some districts.

...ctober 26th.

List of Diseases in October, in the Practice of Dr. Fothergill.

...eumatismus	10	Phthisis Pulmonalis	5	Dyspepsia	6
...bris	2	Marasmus	2	Diarrhœa	3
...rlatina Anginosa	3	Hæmoptoe	1	Dysure	2
...rlatina Maligna	2	Hæmatemesis	2	Erythema Nodosum	1
...nanche Tonsillaris	2	Asthenia	7	Erysipelas	1
...nanche Laryngea	1	Cephalalgia	3	Lichen Simplex	1
...renitis	2	Lumbago	3	Porrigo	2
...ipneumonia	1	Colica	2	Menorrhagia	2
...tussis	4	Gastrodynia	4	Amenorrhœa	4
...sis et Dyspnœa	12	Vomitus	2	Morbi Infantiles	8
...arrhus	8				

MONTHLY CATALOGUE OF MEDICAL BOOKS.

THE Edinburgh New Dispensatory, containing the Elements of Materia Medica and Pharmacy, with accurate Translations of the London, Dublin, and Edinburgh Pharmacopœia. By John Thomson, M.D.—Highley and Son.

A Practical Materia Medica, in which the various Articles are fully described and divided into Classes and Orders, according to their effects; their Virtues, Doses, and the Diseases in which they are proper to be exhibited are fully pointed out: interspersed with practical Remarks and some select Formulæ; to which is added, a general Posological Table, intended principally for the use of Students and junior Practitioners. Second Edition. 12mo.—Highley and Son.

The Anatomical Instructor, or an Illustration of the Modern and most approved methods of Preparing and Preserving the different Parts of the Human Body, and of Quadrupeds, by Injection, Corrosion, Maceration, Distension, Articulation, Modelling, &c. with a variety of copper-plates. By Thomas Pole, Member of the Royal College of Surgeons. A new edition, with additional notes. 12mo. —Callow and Underwood.

An Essay on the Philosophy, Study, and Use of Natural History. By Charles Fothergill. 12mo.—White and Co.

The Art of Preserving the Sight unimpaired to an extreme Old Age, and of re-establishing and strengthening it when it is become weak. 12mo.—Colburn.

Some Account of an uncommon Appearance in the Flesh of a Sheep, with Reflections on the Nutrition of Sheep, &c. By W. Vaughan, M.D. 8vo.—Harding.

A brief Description of the Plague, with Observations on its Prevention and Cure. By R. Pearson, M.D. 8vo.—Underwood.

Floria Glottiana: being a Catalogue of the Indigenous Plants on the banks of the River Clyde, and in the Neighbourhood of the City of Glasgow. By Thomas Hopkirk, Fellow of the Linnæan Society, &c.

A General Account of the Hunterian Museum, Glasgow: including Historical and Scientific Notices of the various objects of Art, Literature, Natural History, Anatomical Preparations, Antiquities, &c. in that celebrated Collection. By Captain J. Laskey. 8vo.—Longman and Co.

NOTICES TO CORRESPONDENTS.

Communications from the following gentlemen will appear in our next Number:—Dr. Uwins; Messrs. D. H. Davies, R. Wright, Charles Mayo, W. Jones; C. W. S.; an Apothecary, &c. Dr. Jeffrey's letter will be attended to.

All Communications are requested to be addressed to the care of the Publisher, No. 1, Paternoster Row.

ON THE COMPLETION OF SETS.

THE Series of the Medical and Physical Journal have for several years past been acknowledged to constitute the most valuable periodical collection of important facts and discoveries in Medical Practice, which exist in any language. It is now Seventeen Years since the work first claimed the notice of the Public, and during this period of unparalleled activity in Scientific and Medical Research, it has been the depository of the communications of the Faculty in every part of the world; while its editors have never failed to glean, from every authentic source, whatever appeared to them to be worthy of the attention of their readers. Hence it is to assume nothing beyond the strict fact to state, that the London Medical Journal now is, and long has been, THE UNIVERSAL GAZETTE OF THE FACULTY, *circulating wherever the English Language is read, and that circulation reflecting, as its natural effect, a universality of respectable correspondence unequalled in the experience of Periodical Literature.*

Under these circumstances, the THIRTY VOLUMES, *of which the entire Series now consist, have acquired a value and importance which the Public, and even the sanguine wishes of the Proprietors, could never have anticipated. At the same time the increasing length and corresponding value of this series have been attended by a corresponding inconvenience to the friends of the work; and many Gentlemen express their regret at the loss and imperfection of Numbers and Volumes, by which their Sets are broken, while they complain of the great expence attending the purchase of the Numbers in which they are deficient, at the full, but necessary price of* HALF-A-CROWN. *On their parts the Proprietors are anxious to make such present sacrifices as may gratify their friends, and perhaps ultimately accord with the interests of the work; they have therefore resolved, during a period of* THREE MONTHS, *i.e. from the 1st of November to the 1st of February next, to vend all the Numbers published prior to the present year at* EIGHTEEN-PENCE PER NUMBER, *or Half-a-Guinea per Volume half-bound.*

This temporary concession on their parts will, they trust, prove agreeable to many of the earliest and best friends of the work, who are also the most anxious to perfect their Series, and serve as an accommodation to the funds of Medical Reading Societies, now established in many districts, part of whose Library-Stock consists in general of parts of the Series of this work.

As the perfecting of Sets will naturally lead to a desire to possess a GENERAL INDEX TO THE WHOLE, *this seems to be a suitable occasion on which to re-announce that a General Index will be put to press as soon as Five Hundred Names are received at the Office of the Publisher, implying a disposition to pay* ONE GUINEA *for the same on its delivery.*